Philosophy of Science

VOL. 2 July, 1935 NO. 3

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Philosophy of Science

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Philosophy of Science and Science of Philosophy

BY

CHARLES W. MORRIS

IT IS proposed to examine the consequences which ensue if philosophy is deliberately oriented around the methods and results of science. That such reorientation has been more or less unconsciously taking place for centuries is evident; the problem demands particular discussion at this time only because the reorientation has gone so far and with such success as to challenge seriously certain past conceptions of philosophy and to demand of philosophers what, if anything, is left for them to do. For present purposes the revolution indicated will be regarded as a *fait accompli*, and the sole concern will be with what alternatives remain open. The conclusions differ from those of certain others who accept the same *fait accompli* only in believing that more alternatives do in fact remain open than they envisage.

We begin then by rejecting any conception of philosophy which regards philosophy as proceeding by methods other than those of science or as obtaining an order of certainty different from that obtained by science. This is essentially the same thing as to

deny the existence of a priori synthetic judgments and any philosophy which rests upon the affirmation of such judgments. If the thesis of physicalism be sound (the thesis that all propositions are intersubjectively verifiable, and translatable into the language of physics) then the accepted standpoint could be simply and literally characterized as the denial of metaphysics, i.e., of any science other than physics. Or putting the case positively, we might say that propositions in philosophy as in science are to be accepted to the degree that they are supported by existing evidence and controllable in terms of further evidence.[1]

It might seem with the acceptance of such a position that philosophers are left without employment, for each of the three dimensions of meaning already appear to be in other hands: the mathematician and symbolic logician have taken the domain of formal or syntactical meaning for their own; the artist and men of affairs seem to be charged with the value and directive aspect of symbols (pragmatic meaning); the scientist has made himself responsible for stating what meanings do in fact hold of things (the empirical dimension of meaning). The philosopher who does not wish to lapse into silence or to turn historian must either justify his entrance into one of the three dimensions of meaning (formal, pragmatic, empirical), or must function in some integration of these dimensions. Let us examine these possibilities in turn.

I. *Philosophy as Logic of Science.* The narrowest of the open possibilities would be to identify philosophy with formal logic, so conceived however that this in turn becomes identical with the logic of science. It is this step which Carnap has in essentials taken. For him logic is "the last scientific ingredient of Philosophy; the extraction leaves behind only a confusion of non-scientific, pseudo-problems."[2] Logic is in turn conceived formally, that is, as dealing with the syntactical structure of actual

[1] The present paper is the third of a series dealing with logical positivism, pragmatism, and scientific empiricism. The two preceding papers appear in the *Proceedings of the VIIIth International Congress*, and in *Erkenntnis*, vol. 5, 1935.

[2] *Unity of Science*, 22. A useful survey in English is found in *Philosophy and Logical Syntax*. In German a survey is given in *Wissenschaftslogik*, and a detailed presentation in *Logische Syntax der Sprache*.

or possible languages (designated as descriptive or pure syntax respectively) in abstraction from both the empirical and pragmatic aspects of meaning. The results of logical analysis will themselves be expressed in terms of the two kinds of propositions found in science: thus they will either be analytical propositions and so fall within the formal sciences, or they will be synthetic propositions and so fall within the empirical sciences. Thus philosophy as "the syntactical analysis of scientific language"[3] eventuates to be sure in propositions, but these propositions are themselves scientific propositions, so that there are no propositions over and above the propositions of science. Philosophy is formal logic, which in turn is the pure or descriptive syntax of the language of science; philosophical analysis is logical analysis. The result is that "the method of logical syntax, that is, the analysis of the formal structure of language as a system of rules, is the only method of philosophy."[4]

One aspect of this view deserves to be stressed, namely, the identification of logical analysis with the analysis of "the formal structure of language as a system of rules." This means that abstraction is made from the relation of symbols in a language to empirical objects and from all psychological and social effects of the symbols. This exclusive attention to the formal dimension of meaning is connected with a conviction that the two neglected dimensions of meaning fall within the province of the empirical sciences. The consequence of the abstraction is that philosophical analysis makes no empirical assertions about non-linguistic objects,[5] and indeed makes no assertions about language other than those about the syntactical structure of actual and possible languages. Thus the tendency has been to replace more and more the earlier tests for meaningfulness[6] by purely formal requirements. The meaning of a proposition is regarded as all the propositions which can be derived from the proposition in ques-

[3] *Philosophy and Logical Syntax*, 7. Cf., *Philosophy of Science*, vol. 1, 1934, 9; *Erkenntnis*, vol. 2, 1932, 238.

[4] *Philosophy and Logical Syntax*, 99.

[5] *Philosophy and Logical Syntax*, 72.

[6] Thus, *Erkenntnis*, vol. 2, 1932, 236: "Ein Satz besagt nur das, was an ihm verifizierbar ist."

tion by the transformation rules permitted in the language under consideration.[7] In general, philosophical activity is concerned with the formal consideration of concepts and propositions. In the case of verification, "the logical analysis of verification is the *syntactical* analysis of those transformation rules which determine the deduction of observation sentences."[8] Similarly, the philosopher-logician says nothing about nature, since that is the province of the scientist, but is concerned only with the language which the scientist uses about nature.[9]

A natural first reaction to such an attempt to find "an exact method of philosophy" is the feeling that the price paid for exactness is too high. It turns out, however, that the field is much more extensive than would at first be suspected, and I do not see how anyone can follow the discussions of formal meaning, of the place of rules in determining formal necessity, of the multi-valued logics, of the foundations of mathematics, of physicalism and the unity of science, without being both impressed and thankful. Nor can it be doubted that the identification of philosophy with the logic of science would give to philosophy a place of significance which would keep it in the closest relation to science and yet not obscure its own function. As a minimal definition of philosophy it is ingenious, excellent, and praiseworthy. The important question is not whether philosophy can do less than fulfil this rôle, but whether it can also do more. To make this "more" initially plausible, one comment may be added. If the syntax of a given language is studied, the resulting statements are empirical and in the last analysis fall within the science of linguistics, while if the syntax of a proposed language is studied

[7] See especially the statement on pages 56–57 of *Philosophy and Logical Syntax*. Cf. *Philosophy of Science*, vol. 1, 1934, 12. The implication of this shift for the problem of truth is obvious: it involves passing from a correspondence view to a form of coherence view, i.e., a true proposition is simply one compatible with or unifiable with the accepted propositions of a science. The evolution in the views of truth is discussed in an article by C. G. Hempel, "On the Logical Positivists' Theory of Truth", in *Analysis*, vol. 2, 1935. This shift is the price paid for neglecting other aspects of meaning than the formal when certain difficulties in the earlier empirical formulations of meaning were encountered.

[8] *Philosophy and Logical Syntax*, 83. Cf. *Unity of Science*, 25.

[9] *Philosophy and Logical Syntax*, 84.

in a form such that the resulting statements are analytical, the work of the philosopher is hard to distinguish from the mathematician, and in any case does not result in really saying anything about anything. If the philosopher is thus bound to trespass upon either the domain of the natural scientist or mathematician, why be too squeamish at the start in trying to define the function of the philosopher over against the scientist, since in the end philosophy and science will be found to overlap.[10]

II. *Philosophy as Clarification of Meaning.* The most natural extension of this minimal definition of philosophy would be to apply the method of logical analysis to all concepts and to all dimensions of meaning. As to the first point (the extension to all concepts), Carnap at times suggests that his conception of logical analysis covers "all assertions of science and of everyday life."[11] But actually he does very little with the latter assertions and can do very little because the assertions of daily life are shot through and through with judgments of value, and these are for Carnap technically without meaning. The instance is given that the statement "Killing is evil" is a command in misleading form, and that no deductions about future experience are possible from it.[12] Now this is obviously a cavalier handling of complicated questions, because it is not impossible that judgments of value are, as Hume suggested, a species of judgments of fact,—indeed it is precisely such questions that logical analysis should be able to determine. It is not surprising that other persons, such as Lewis, have been able to write that "philosophy has for its task such analytic depiction of the a priori—to define the good, the right, the true, the valid, and the real."[13] On such a

[10] This is particularly true if it be recognized that the formal sciences are merely a part of empirical science. Although analytic and synthetic propositions may be distinguished, the determination of the status of a given proposition or the determination of what propositions really do follow from others according to specified transformation rules are of course empirical in that observation upon symbols is involved. See my article, "The Relation of the Formal and Empirical Sciences within Scientific Empiricism," *Erkenntnis*, vol. 5, 1935.

[11] *Philosophy and Logical Syntax*, 9.

[12] *Ibid.*, 24–25.

[13] "Logic and Pragmatism," *Contemporary American Philosophy*, vol. 2, 48.

view philosophy is still logical analysis, but the tone is not so formalistic, the range of concepts and propositions considered is wider, and the interest in meaning is wider than the interest in formal meaning. This conception of philosophy as furnishing and applying a method for the clarification of ideas has been of course continuously represented in American pragmatism from its origin to the present. It is also championed vigorously at the present by one wing of the *Wiener Kreis*, especially by Wittgenstein, Schlick, and Waismann. Wittgenstein, in a passage in the *Tractatus* quoted too often to need repeating, has spoken of philosophy as an activity. Those influenced by Wittgenstein stress the view that this philosophical activity does not issue in propositions: philosophy is not a science nor an attempt at the synthesis of the sciences, but is simply a process of conceptual and propositional clarification. Thus in theory the interest in this group is not so strongly centered around science or around formal logic, though of course the actual relation of the members to these fields is very close. The interest is rather in taking an unclear concept or proposition, considering it in relation to various usages and situations, making the necessary distinctions which are called for,—until finally the unclarity is resolved. The result is neither a proposition which claims truth nor a proposal as to how words are to be used in the future, but simply the clarification of a difficulty and the return to the unproblematic (a phrase reminiscent of Avenarius, Peirce, and James). Each clarification is a job done, and philosophy will have work to do as long as such clarifications are needed. Philosophy has then no special subject matter about which it aims to communicate truth; it is rather the dimension of ideational clarification present in all reflection. Einstein, for example, was philosophical while he was attempting to clarify the concepts of space, time, and simultaneity. The general result, as Schlick says, will be that in time "no more books will be written about philosophy, but *all* books will be written in a philosophical manner."[14]

Because of the refusal of members of this group to state proposi-

[14] "The Future of Philosophy," *Proceedings of the VIIth International Congress of Philosophy*, 116.

tions which might be construed as the holding of theses, it is difficult to be clear as to what it means to clarify a meaning,—for no general theory of meaning is given. It is perhaps fair to say that as in Carnap's case there has been a shift in emphasis from the empirical aspect of meaning to the formal dimension, but the shift has not been so extreme.[15] Wittgenstein and Waismann no longer stress language as a reflection of the structure of the world of existence but stress rather the central importance of certain relatively arbitrary rules of usage as determinative of the meaning of the term or proposition in question. Hence one hears much of the importance of a "general grammar" and at times a preference for such phrases as "the method of grammatical analysis" instead of the label of "logical positivism." The consequent shift in the center of logic from tautology to rule (as Waismann phrases it) recalls the central place in logic which Peirce had given to "leading principles."

Peirce, however, took a step which I believe it is necessary to take if the alternative now being considered is not to collapse into the first conception of philosophy: he saw that the corollary of an interest in the clarification of meaning was the development of a general theory of meaning, and the conception of logic itself as general semiotic. To take this step does of course mean to state theses, and it does mean that philosophy is brought within the field of the existential sciences, using, like all sciences, logical analysis in its activity. The reluctance to take this step is understandable, for it seems to lose the neatness of the demarcation of science as statement of fact from philosophy as analysis of meaning. One can perhaps admit that a functional division of labor along these lines is possible, and indeed that it is approached in such distinctions as those between pure and applied mathematics, experimental and theoretical physics (similar distinctions

[15] This is especially true of Schlick. See *Les énoncés scientifiques et la réalité du monde extérieur*, 24 ("Bref, l'indication des conditions dans lesquelles une proposition est vraie *se confond* avec l'indication de son sens. Il est tout à fait inutile de chercher plus loin"). His article "Facts and Propositions" (*Analysis*, vol. 2, 1935) is an explicit defense of empiricism against the newer formalistic extremes. In my opinion this resurgence of formalism—empiricism controversies is an evidence that the *Wiener Kreis* has no sufficiently general theory of meaning.

seem to be advisable in all of the sciences). But in general it is difficult to see why a component of all reflection should be singled out as the philosophical component, and certain it is that scientists are not going to turn over the task of determining the meaning of the terms they use to any other group of persons.[16] Too much mystery is thrown around the analysis or clarification of meaning: the meaning of a term is completely specified when it is known what objects the term designates, what expectations it produces in the persons for whom it has meaning, and what its connections are with other terms in the language of which it is a part. The determination of the first gives the empirical dimension of meaning, the determination of the second gives the pragmatic dimension of meaning, and the determination of the third gives the formal dimension of meaning. The point not to be overlooked is that the determination of all these dimensions is an empirical process: the fact that the formal dimension of meaning is not the empirical dimension does not contradict the fact that the determination *of* the formal meaning is itself an empirical process. Logical analysis is itself one kind of empirical analysis, namely, the determination of what we are committed to within the domain of symbols if we allow a certain habit in the use of symbols to proceed, i.e., if we follow a certain syntactical rule. Since this is so, there seems to be little reason for not expanding philosophy into a general theory of symbolism. Unless this is done, and the pragmatic and empirical dimensions of meaning considered on a par with the formal dimension, no significant alternative is presented to the conception of philosophy as formal logic. Otherwise—silence and the unproblematic!

III. *Philosophy as Empirical Axiology.* Professor Schlick has said that his conception of philosophy is nearest to the old conception of philosophy as "wisdom of life,"—for the sage is the person who sees most clearly the wider meaning of his words and activities.[17] Professor Schlick himself always kept this

[16] If indeed the meaning of a proposition is all that can be deduced from it by both logical principles and scientific laws the fitness of the philosopher to determine meanings is by no means clear.

[17] *Les énoncés scientifiques*, etc., 13–14; *Proceedings of the VIIth International Congress of Philosophy, 113–114.*

implication of his conception of philosophy in mind, but by and large his group has looked at science from the point of view of the scientist, and not with the vision of one intent upon the bearing of the scientific attitude and results upon human culture. And here arises a possible conception of the philosopher's task. The resulting conception of philosophy is one which Dewey has made peculiarly his own. It is clear that it would require two kinds of development: on the one hand it would be necessary to formulate a general theory of science as an institution and science as a habit of mind, and not to be content solely with a formal analysis of the language of science; and secondly, it would be necessary to elaborate the implications for all the domains of value of the acceptance and extension of the methods and results of science to the widest spheres of human life.

Dewey has been peculiarly sensitive to the instrumental relation of symbols to the life of the individual and the community. He has envisaged intelligence as a tool in the service of some value, and science as coördinated and institutionalized intelligence. To him science is rich in potentialities for the control of human life, and for the enrichment and emancipation of the individual mind. He is impressed by the gap between the possibilities of this "most potent social factor in the modern world" and the slowness of its extension into the fields where value judgments hold sway. It is in these terms that he can write that "the great scientific revolution is yet to come,"[18] and can conceive the task of philosophy today as the extension of the method of freed intelligence into ethical and social domains. And this extension is of course something advocated, and as advocate the philosopher has himself turned moralist. Yet it is important to emphasize the difference between this conception of philosophy and the ordinary systems of admonitions which pass as philosophies of life. In this positivistically toned equivalent of the older conceptions of philosophy as vision, vision has lost both its dogmatism and its fugitive emotionalism and has been tempered by the attitudes and results of science. But in being

[18] *Philosophy and Civilization*, 329.

tempered it has not lost its imaginative character.[19] Here is science crowned by and ministering to social vision. Dewey has written: "One of the few experiments in the attachment of emotion to ends that mankind has not tried is that of devotion, so intense as to be religious, to intelligence as a force in social action."[20] Dewey's own life has been the devotion to that experiment.

Dewey feels that such a conception of the rôle of philosophy is supported by an examination of the rôle it has always performed. To him philosophy has always mediated between fact and value, between "a stubborn past and an insistent future": "The work of philosophizing," he writes, "is the old and ever new undertaking of adjusting that body of traditions which constitute the actual mind of man to scientific tendencies and political aspirations which are novel and incompatible with received authorities."[21]

To give content to the general doctrine it was required to turn to detailed analyses in various fields of value and contemporary social problems, and to delineate and generalize the essential features of the scientific habit of mind. The latter task constituted Dewey's contribution to logical theory, while his results in the former fields are spread out in numerous books and papers on ethics, esthetics, law, education, religion, and social philosophy. It is clear that the method includes the use of logical analysis of the preceding conception of philosophy, but applies it especially to the analysis of value concepts. And here the pragmatic dimension of meaning is brought to the forefront. The third conception of philosophy thus turns out to be an empirically oriented axiology culminating in a deep concern for the ethical and social potentialities of that type of intellectual procedure which we have come to call scientific.[22]

[19] Dewey often states this in extreme form: "Meaning is wider in scope as well as more precious in value than is truth, and philosophy is concerned with meaning rather than with truth" (ibid., 4); "the scientific factor, the element of correctness, of verifiable applicability, has a place, but it is a negative one" (ibid., 10).

[20] *A Common Faith*, 79.

[21] *Philosophy and Civilization*, 3–4.

[22] It is not uncommon to find "scientific" philosophers looking down upon Dewey's work. For the most part they fail to see that they are merely dealing with different

IV. *Philosophy as Empirical Cosmology.* The three views of philosophy we have now discussed (philosophy as *ancilla scientiae*, as *scientia sermocinalis*, and as *ancilla hominis*) present between them non-metaphysical equivalents for the traditional domains of logic and axiology. The question now arises as to whether there is any non-metaphysical (or positively put, any empirical) equivalent to what has usually been called metaphysics (now often spoken of as speculative philosophy).[23] The first three alternatives present what in a wide sense of the term could be called a philosophy of science, but nothing that could be called a science of philosophy; and they stress the formal and pragmatic dimensions of meaning rather than the empirical dimension.

The ideal of introducing greater exactness into philosophy is an ideal to which lip-service is widely paid. But there are some who feel that serving this ideal does not mean the denial to philosophy of any synthetic or systematic character. Husserl, for instance, does not think the logical positivists deal with genuinely philosophical questions, but that his own phenomenological method supplies for the first time the basis for a scientific philosophy. Peirce, James, Dewey, and Mead have all defended in one way or another an empirical equivalent to metaphysics in the form of a search for the generic features of all experience. This conception is formulated explicitly in Whitehead's *Process and Reality*: "Speculative philosophy is the endeavor to frame a coherent, logical, necessary system of general ideas in terms of which every element of our experience can be interpreted."[24] He calls this method the "method of descriptive generalization,"[25] and delineates it as "the utilization of specific notions, applying to a restricted group of facts, for the divination of the generic notions which apply to all facts."[26] The test is empirical: "the test of

aspects of science and with different dimensions of meaning. Scientists should not misunderstand those who have accepted science to the hilt, who see its cultural implications, and who are concerned with the existence of a society in which science and scientists may continue to live!

[23] The epistemological aspects of the problem are considered in an article, "Pragmatism and Metaphysics," *Philosophical Review*, vol. 43, 1934.

[24] *Process and Reality*, 4. The term "necessary" is for our purposes unnecessary.

[25] Ibid., 15–16.

[26] Ibid., 8.

some success is application beyond the immediate origin." So seen, "metaphysical categories are not dogmatic statements of the obvious, they are tentative formulations of the ultimate generalities."[27]

It is the fashion of some positivists today to look askance at Whitehead's later work, and I must admit that Whitehead, as Peirce, seems often to desert his method and to talk metaphysics in the sense here rejected. But it must also be admitted that the method proposed for philosophy is simply the hypothetical-deductive-observational method of science, generalized in the attempt to get a system applicable to all experienced reality whatsoever.

The literal acceptance of this method would mean an acceptance for philosophy of the criteria proposed by Newton for science.[28] It would mean that after a merciless process of logical analysis in which pseudo-questions were eliminated, proposed answers to the remaining questions would be evaluated in terms of the evidence at hand, and answers not supported by evidence would be rejected as vain imaginings.

If we accept this conception of philosophy as a legitimate alternative, we come in an unsuspected manner to the kernel of truth in the ancient conception of philosophy as the queen of the sciences (*regina scientiarum*). So conceived, the task of philosophy is to erect a conceptual scheme of such generality that it is confirmed by all data. It differs from science in the narrower sense only in generality, and not in method nor in the security of the results. Its data is all data, whether found in the special sciences or in the ordinary world of perception and action. Its problems are all problems relevant to the completion of its

[27] Ibid., 12.

[28] As given in the *Principia*: "Rule III. The qualities of bodies, which admit neither intension nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever. Rule IV. In experimental philosophy we are to look upon propositions collected by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypothesis that may be imagined, till such time as other phenomena occur, by which they may either be made more accurate, or liable to exceptions." Needless to say, Newton did not follow these principles in his own philosophy—and the results speak for themselves.

specific task. Its interest in the clarification of meaning is only in the clarification appropriate to the erection of its conceptual system. On the formal side, the undefined terms of such a system are the categories, and the primitive propositions are those hypotheses whose logical consequences are to be investigated; on its empirical side the system is a cosmology subject to control by all available data. Philosophy as empirical cosmology differs from previous metaphysical systems as science differs from early science, that is, from magic.

The most natural objection to this view of philosophy would be to hold that what has been called empirical cosmology falls within science.[29] This reply is not without force, since it could be held with some plausibility that most, if not all, significant unifications of knowledge have come and must come from below and not from above, from scientists and not from philosophers. But actually this reply begs the question, for if by definition we include all knowledge within science, then it is analytically true that philosophy as distinct from science has nothing to do with knowledge.[30] On this usage it becomes even difficult to see why knowledge of the language of science should in any way be equated with philosophy. It is, however, equally possible to maintain that at the level of the widest system of knowledge the distinction between philosophy and science vanishes: *a unified completed science and an achieved philosophy would be identical.* But until that goal is reached it is possible to distinguish between conceptual systems progressively adequate to specific domains of experienced existence, and the attempts to formulate a system adequate to all domains whatsoever. This does not mean to dictate to science, but to use scientific results and the rich field of common life in the service of the most generalized science, that is, philosophy. At any given time, of course, a philosophy

[29] See Carnap, *Philosophy and Logical Syntax*, 15–16.

[30] It would be interesting to consider cases where philosophers have actually broken ground for science—such as Democritus and atomism; Cusanus and relativity of motion; Leibniz and unified mathematics and science; Peirce, James, Reichenbach and objective indeterminism. The final rejection or vindication of such insights is of course important and laborious—but this should not lead to a condemnation of insights as such, especially when they are of such a type as to be empirically controllable.

is partial and liable to change, as is any particular science, but there can be no opposition of science and philosophy, and in one case as in another the results are not "vain imaginings," but generalizations empirically grounded and empirically controllable. Philosophy could be as continuous and as progressive as the existing sciences. And at any given time, one could hold with Whitehead that "the useful function of philosophy is to promote the most general systematization of civilized thought,"[31]—a systematization that is to be changed or discarded as the evidence demands.

Conclusions. In conclusion a few remarks upon the relation of these four conceptions of philosophy are needed.

First, with reference to the ideal of cognitive certainty, it may be said that the results become less and less certain as we pass progressively from the first alternative to the fourth. In so far as cognitive certainty is taken as the norm, the activities of philosophers should be concentrated upon the fields of work defined by the earlier conceptions. Although this permits work at all levels, it does imply that in general an emphasis upon the logic of science, logical analysis, and a general theory of meaning is desirable at this time.

Second, with reference to other criteria than cognitive certainty diverse emphases are possible. Thus with reference to an individual's special ability or with reference to the special needs of a particular historical epoch other emphases may be desirable. In the present, for example, it could be plausibly argued that philosophy should take seriously the ethical and social responsibilities which conception three demands.

Third, to some persons the view of philosophy discussed in connection with Dewey may seem at odds with the other theories since it has an element of practicality and concern for social issues absent in the others. If indeed "vision" is something simply added to knowledge, it would be understandable why philosophers devoted to science should question this additional step. If ideas are only descriptions this questioning is defensible,

[31] *Process and Reality*, 25–26.

but if ideas do function as predictions, if they do have irreducibly a pragmatic or action dimension, then philosophic vision is not different from scientific prediction, but is simply the most general expectations appropirate to a system based upon all the data at hand. Philosophic wisdom is merely the wisdom accompanying the widest knowledge. Science reveals no absolute break between theory and practice, and there is no clear reason why the situation should be different in philosophy.[32] Meaning at the level of philosophic generality has its pragmatic dimension just as have the meanings at other levels. It is important to distinguish dreams and fancies from expectations and proposals based on whatever data is available and controllable by new data obtained in use. It would be a signal instance of ethical irresponsibility to relinquish the demands which the need for a philosophically disciplined imagination imposes, and to turn the world over to the exclusive control of dreamers, adventurers, men of action, and technicians. All choices of rules for action can best be made in terms of the purposes to be effected and the data to be considered. The philosopher aware of social responsibilities is simply carrying out on the widest scale the same type of action which the logician exemplifies in the selection of rules of operation, and which the scientist illustrates within a particular domain in his search for the conceptual system most adequate to the facts at hand and to the most exact determination of expectations as to the future. Philosophy is the most general science and the widest vision—and the one because it is the other.

Fourth, it is important to note that the first, third, and fourth views, often wrongly considered as mutually exclusive alternatives, deal with complementary and not opposed aspects of the meaning situation. The implication of this fact is that *formalism, empiricism, and pragmatism are complementary phases of the scientific temper.* I propose to recognize this fact by designating the implied philosophical attitude as *scientific empiricism*, the

[32] "Whatever is found in 'practice' must lie within the scope of the metaphysical description. When the description fails to include the 'practice,' the metaphysics is inadequate and requires revision. . . . Metaphysics is nothing but the description of the generalities which apply to all the details of practice." (*Process and Reality*, 19).

term "empiricism" indicating the acceptance of concepts and propositions in proportion as they are based upon and controllable by evidence, and the term "scientific" suggesting not merely that science is the recognized focus of orientation but also that whatever formalistic, pragmatic, or cosmological factors are operative in the scientific enterprise are compatible with this version of empiricism. The proposed formulation thus accepts and reconciles all four of the previously considered conceptions of philosophy. It should not be impossible that with the passage of time and with the acceptance of the coöperative responsibility which the program of scientific empiricism entails present philosophical theses (such as the defense of realism) will compare with the future status of such theses much as Greek atomic theory compares with the atomic theory of the present time.

Fifth, each of the three dimensions of meaning has developed its own typical form of expression: mathematics is the language of possibility, science is the language of fact, art is the language of value. Philosophy may in turn be looked upon as the language of languages. This expression itself has a double aspect: it suggests a language about languages, and in this sense philosophy is general semiotic (general theory of symbolism); it also suggests the most comprehensive of languages and in this sense philosophy is general science, and possesses the values which go with such generality. When the phrase is so understood, all four of the conceptions previously considered are incorporated in the conception of philosophy as the language of languages.

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Metaphysics for Positivists

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WHAT is metaphysics? There is real danger of taking the meaning of this term too much for granted, and of leaning too heavily upon an undefined word in the very act of dismissing metaphysics as word jugglery. Metaphysics is not, as I conceive it, the study of the wholly transcendent or supersensible. It may sometimes have been so conceived, and this is, indeed, one reason why some of its alleged results are so unsatisfactory—or, as positivists would say, meaningless. Such a meaningless "metaphysical" idea is the mediaeval doctrine of God as a power acting upon the world but not reacted upon by it, as purposive but non-temporal, loving but "impassive," etc. Such is the windowless monad of Leibniz, connected with other monads only by pre-established harmony. Such is the Aristotelian idea of substance as that in which other things inhere but which itself is in nothing (apparently not even in the world or nature). The history of metaphysics is, indeed, strewn with failures to achieve meaning. But does this mean that metaphysics has scored no successes? Surely we must remember that human success is invariably of the fumble-and-correction type. Ideas such as those mentioned may be meaningless as they stand, and yet have potential meaning in that some modification of them might render them significant conceptions. We must also remember that mankind now possesses new instruments of logical analysis, and that these instruments may prove useful in metaphysics—not, as logical positivism holds,

with the result of banishing all metaphysical ideas, but of correcting their erroneous aspects.

Metaphysics is the study of ideas universally applicable. These ideas define being *qua* being, the widest universe of discourse, or the universe of "all time and all existence." Thus metaphysical does not mean behind or above the physical (or the observable), but all through the physical (or observable), and all through everything else, if anything else there be. In terms of how we know them, metaphysical ideas may be defined negatively as those known in some other way than by ordinary induction, or by purely formal (mathematical) deduction. Thus the principle of induction itself—as positivists admit—is not known by ordinary induction or by mere mathematics. Moreover, no statement concerning all time could be arrived at by induction. The laws of science used to be called "immutable," but no jot of inductive evidence for this conception of them is available. Does it follow that the question, What laws, if any, are forever true, is meaningless?

There is another possibility. Is not the clue to an answer to the question, what is immutable, to be found in the reflection that whatever change may do it cannot alter change itself? Time, that is, is itself eternal. But perhaps—you will say—time can be conceived to end, if not to change. This depends upon whether "existence" means anything in abstraction from time. I can find no imaginable meaning; in positivist language, no conceivable protocol meaning. And theologians have generally held that the attributes of the timeless God are chiefly if not solely negative. Thus there is reason for taking "eternal" to mean "whatever is essential to time as such." Eternal truth is simply the content of the idea of time. Is it meaningless to ask what is the content of this idea? You may answer that perhaps many ideas of time are possible. Even so, we can ask what they all have in common. Besides, the content of the idea of time is partly fixed by the functions we require this idea to fulfil in relation to other fundamental ideas, such as actual, possible, individual, universal, space, etc. It is because these other ideas also lose their meaning if time be supposed absent that the abstraction from time was above declared impossible.

Metaphysics is the attempt to fix the content of basic ideas in relation to one another.

Of what logical type are metaphysical judgments? Take the principle of induction, the reality of a cosmic order thanks to which predictions are possible. If, as has been asserted, this principle is pragmatic, then at least a pragmatic metaphysics is possible, whether or not this is its only conclusion. The form of its reasoning would be: if there are values which are presupposed in all possible valuation, these values must be pragmatically unquestionable. The possibility of prediction need not be the only such universally presupposed value. Moreover, the idea of a truth which must be believed but for which no evidence is attainable should not be lightly accepted.

According to positivism, philosophy is concerned with languages. Can it refuse to consider the question, what meanings are common to all possible languages? The definitions of these meanings will have universal, that is, metaphysical, relevance. Of course one may say that they will be universal meanings rather than universal truths, but the distinction is itself meaningless. For it is only with non-universal meanings that significance is separable from truth. Specific ideas may represent possibilities, not actualities, because they are correlated with specific alternatives; but the most general ideas are those which define the entire system of possible alternatives, but which themselves have no alternative. If they have meaning, this meaning can only consist in their describing reality; indeed, they will define "reality" as such, i.e., in its eternal, universal traits. There is no possible untruth of the most general ideas, but only the possibility of confusedly grasping their meaning. Failure to grasp metaphysical meaning or truth involves either the defect of vagueness or the defect of inconsistency. The remedy—never entirely efficacious it must be admitted—is to relate the metaphysical ideas into a system in which each concept helps to define and is partly defined by all the others. Circularity of definition is not necessarily fallacious. For this we have the authority of C. I. Lewis.[1]

[1] *Mind and The World Order*, pp. 81–82.

Allied to the self-contained character of metaphysical definition is the intrinsicality of metaphysical evidence. The "coherence theory" of truth is admittedly unsound as applied to specific or inductive truths, truths with significant alternatives, yet it is sound as applied to generic or necessary truths. For the denial of a necessary truth can only be a necessary error, and what could this be but a contradiction in terms, an incoherent expression? It must also be remembered that it is not a question of mere ideas for which embodiment in some concrete experience must be found. For metaphysical ideas by virtue of their generality are always embodied in experience, including the experience of the metaphysician at every moment of his thinking. He may indeed fall into mere ideas in the sense of mere words without meaning; but if his words have meaning, this will be because his experience presents something to which the words refer. It is his business to find experiential basis, as well as systematic coherence, for his ideas. Whether or not he has succeeded will be judged by others with reference to their own experience, and their own methods of testing coherence.

But once more we confront the question, can anything really universal be arrived at by human beings? The answer is that "really universal" is itself a human concept, and cannot be used to demonstrate the necessarily non-universal character of human conceptions. To say that things may exist which do not correspond to our idea of existence (as entering into this assertion) is to contradict oneself. But surely human imagination is limited, surely there may be things the very possibility of which is unknown to us! Is not the blind man unable to conceive color? Perhaps, but what the blind man here lacks is not generality but specificity of meaning. He knows very well what in general color sensations must be: for he knows they are sensations, that they have qualities, relations of space and time, intensity, etc. The fact is that in order to entertain strictly universal meanings it is not necessary to have all possible experiences, but only experiences of some or *any* kind, plus the capacity to abstract, to create symbols for abstractions, and symbols for symbols—in short, to explicitly generalize. If a fish turned theologian, it

would not conceive God to be a great fish (in spite of Rupert Brooke's poem to this effect), any more than Aquinas conceived him to have limbs. A thinking animal, an abstracting and speaking animal, is limited not by virtue of being confined to the specific, but by virtue of being confined to the generic, plus only a small number of species out of the total. As Spinoza rightly said, we have an idea of God—any God there can be—and an "adequate" idea even, in the sense that it is as general as omniscience, but an inadequate one, in that it is abstract and vague when compared to the concrete and distinct generality of omniscience. The real difficulty is not in forming the most general ideas, for we cannot help forming them, but in distinguishing them (1) from each other and (2) from the more specific ideas with which they are entangled. The first difficulty is seen in its acuteness when we reflect that the universal aspects of existence may not be discrete but may form a continuum. In fact, it is a conclusion of metaphysical inquiry that they do so (Peirce's doctrine of Synechism). It follows that there is no one final system of metaphysics, but an inexhaustible variety of ways of "carving up" the ultimate continuum. Neglect of this fact that the ultimate truths lack the discreteness of discursive thought is a cardinal weakness of Husserl's doctrine of the Wesenschau, with its enlightening but over-simple conception of metaphysical insights. There is truth in C. I. Lewis's contention that the ultimate cannot be captured in words; this much truth, that no metaphysical analysis will ever be the absolutely satisfactory and final one, but will be more or less relative to the particular needs of the culture that produced it.

The second difficulty, that of distinguishing the generic from the specific ideas, is met by systematically confronting every idea with the attempt to conceive its alternative. Thus, as we have already seen, theology performed a valuable experiment by attempting to treat temporal being as one species of being and timeless being as another. But the more one tries to get meaning out of this latter notion, in connection with other general ideas, the more one sees its meaning vanish, either into sheer vagueness or into contradiction. If, on the other hand, one treats time as

generic, one can, by certain considerations recently set forth (Bergson, Peirce, Whitehead), find a place for all the fundamental ideas in a way which the older theologians did not anticipate, even as a doctrine to be rejected. Thus the alleged generalization beyond time is revealed as illusory. But if a physicist were to say that quantum mechanics is a strictly universal description of being, the answer would be that the very instruments of analysis which enable us to define the quantum can just as easily be used to define alternatives, the only difference being that the latter would not fit experimental facts. A race which could not generalize beyond Planck's constant could not conceivably have discovered that constant. Physics selects among possible hypotheses, so that it is always certain that no physical theory represents necessary and eternal truth.

It appears from the preceding considerations that although metaphysical reasoning is not induction in the ordinary sense, it has an inductive aspect. For induction is reasoning from samples, which in ordinary induction are taken from perceptual experiences, but which in metaphysics are taken both from perception and from imagination ("ideal experiment"). Imaginative samples prove nothing in science (apart from pure mathematics); but might not induction from the whole range of imaginative variations upon perception which the thinker can invent, or discover in imaginative writings, including the history of thought, support an inference of the form: all imaginable entities, or all conceivable protocol statements, have such and such properties in common, ergo the widest concepts, such as being, can only refer to these properties? Of course we could never say that we have exhausted all imaginative possibilities; but by classification according to some rational principle, such as Peirce's arithmetical list of categories, we might have reason to think that we had exhausted the major general classes, the neglected species involving only minute distinctions, and hence the sources of no gross errors, as the blind man makes no gross errors in philosophy by virtue of his blindness. If, as suggested above, ultimate truth is continuous, then to know it is to know its dimensions, and these can be discerned without experiencing

every point on the continuum. From a few points extrapolation and intrapolation will yield conceptions of the dimensional contrasts.

According to this conception of imaginative induction, metaphysical judgments are both analytic and synthetic, though in different senses. They are analytic or a priori in the sense that they apply to all time, to actual as well as possible states of affairs, and hence arrive at necessary rather than contingent truth. Yet metaphysical judgments are synthetic in the sense that it is always only probable that we have approximated to a correct understanding of necessary truth. Even in arithmetic it is merely probable that no mistake has been made in a given course of calculation. But in metaphysics the lack of absolute certainty goes much further. For even imaginative sampling can never be absolutely "fair," and at best our results lack the definiteness of purely mathematical ideas.

On the other hand they do not lack definiteness altogether. It is true, for example, that the idea of time is based upon an intuition, not a mathematical diagram or expression. Consulting this intuition some metaphysicians report that through all imaginable variations persist as characters of "time" the mass of settled or determinate events, the "irrevocable" past, contrasted with the unsettled, undetermined class of events constituting the "open" future. It follows, say these metaphysicians, that the concept of determinism, which implies that future events are in reality determined, is a contradiction in terms. This contradiction is a clash of intuitions, not of symbols manipulated according to rules. In so far it is not contradiction in the strict logical sense at all. Nevertheless, mathematical patterns can be fitted to the intuition of time, and to the idea of determinism, and the contradiction in question thus made strict and formal—only, however, on pain of making the relevance of the mathematical forms to the intuitions (imaginative experiences) itself a matter of intuition or of inductive probability. Thus metaphysical judgments are complex, and are not identical in kind with any of the usual classes of judgments, empirical or a priori. They do conform, however, to the most general prin-

ciple of scientific knowledge, the application of mathematical forms to concrete intuitions or immediate data. (The Peircean doctrines of the arithmetical structure of the categories, and of their continuous, hence potentially geometrical, character, constitute some of the most remarkable steps in this direction ever made.)

Although ordinary induction cannot establish true metaphysical ideas, it may help to discredit false ones. This relation between metaphysics and inductive science is well illustrated in the history of the idea of substance. Modern physics and biology have shown that, in this cosmic epoch, at any rate, the substances admitted as such by Aristotle are really compound, consisting of parts which are themselves substances, that is, individuals. The rock has its atoms, the man his cells, and the cells their molecules. The compound individual, a non-Aristotelian concept, is made virtually compulsory (for this "cosmic epoch") by experimental facts. But it was all along and eternally compulsory for metaphysical reasons. For the universe is necessarily individual, *and* it is necessarily compound. Spinoza pointed out that all individuality short of the universe is relative; but critics of Spinoza can show that even the individuality of the universe presupposes and is relative to individuality in its parts. There are many reasons for generalizing this relativity of substantial unity.

The idea of the compound and relative individual delivers us from two intellectual nightmares. The first is the nightmare of absolute atomism, which makes all higher levels of individuality mere shorthand for the lowest level. This view not only makes the universe as a whole metaphysically unintelligible, but it also contradicts the most certain of all facts concerning individuality, which is that the very meaning of the word depends on the experience we human beings have of individuality on a superatomic level. Consciousness is not a mere composite; its unity is given, and is the basis of all concepts of unity. Among the corollaries of this truth is the conclusion that both those who seek to deny, and those who seek to assert, the freedom (in the sense of spontaneity) of the human individual upon the basis of quantum

mechanics alone are mistaken. For if a man is not merely many electrons, or many molecules, however organized into coöperating groups, but is also a single unitary individual (in some relative sense or to some degree) then the laws of quantum mechanics are only relatively true as applied to the human body, indeed (by analogous reasoning to be considered presently) as applied even to a single cell or protozoa. Quantum uncertainty may, as some hold, be statistically nullified in the body, in spite of the amplification of small effects by the nervous system, but the human individual as a unit may nevertheless possess its own quantum of uncertainty, indeed must possess it, if the analysis of time above hinted at is correct.[2] The second nightmare of absolute substantialism is old-fashioned theology which, at least by implication, made all levels of individuality but the highest mere appearances or passive effects of the latter, mere puppets of deity. This is the opposite error; the universe as a unified individual may possess maximal, supreme power, but it cannot possess all the power, for then it would unify nothing, just as absolute atoms would not even be parts of the universe. Materialism and old theism are equally superseded in the concept of compound individuality.

Also superseded is dualism. Carnap's doctrine of the unity of science, which does not assert that all sciences must discover the same empirical laws (a question which he leaves open, but which metaphysics answers, as just suggested, in the negative), but which does assert that all science must speak the same language, is sound metaphysics, even though it eschews metaphysics. For there are metaphysical principles at all only if all existence does have common features, expressible in common terms, or in one language. The only problem is the adequacy of the universal language proposed by Carnap, the "physicalist" language, or the language in terms of space-time structures. This language is universal only if we admit that experience is for scientific purposes identical with its physiological-behavioristic correlates, which are all that the physicalist language can express. Two

[2] See also my article on *Contingency* in the *Jour. of Philos.*, 29 (1932), pp. 421ff., 457 ff.

arguments seem to be offered for this identification. First, whatever about experience cannot be translated into the physicalist language is necessarily purely private and incommunicable. Only physical structures are intersubjectively verifiable. Second, propositions in the language of experience are formally equivalent to certain propositions in the objective language, since by psycho-physical laws the latter can be deduced from the former and vice versa.

The first argument is valid only upon certain assumptions, which are of a metaphysical nature, involving, I believe, a subtly false metaphysics. To verify the feelings of others directly, or without translation into physical terms, we should—as a positivist has said—have to *be* those others, while yet remaining ourselves. This is held to be a contradiction. Is this reasoning not precisely a metaphysical argument? And the metaphysician knows where he is, namely back at the discarded conception of absolute individuality. For it is only this conception that is contradicted by the hypothesis. The conception of individuality as compound and relative suggests a different conclusion, the conclusion that the mutual exclusiveness of selves is relative and not absolute, and that therefore the language of experence may not be absolutely private, even apart from its translatability into the language of space-time.

It is true that the experiences of other human beings are not clear and distinct data in our own experience. But surely human beings are not the only experient individuals. The higher animals lack the language medium for communicating their feelings to us, but we do not seriously doubt that they have them. Nor is this conviction due solely to the fact that the expressive cries of animals constitute a sort of sub-language common in a measure to all the higher animals not excluding man. It is due also to the close structural and behavioristic analogies which irresistibly convey the impression of certain emotions and sensations as roughly common to the vertebrates at least. The question arises, at what point in the downward series of forms do we draw the line and say, here such a thing as feeling (memory, desire, etc.) absolutely ceases? Certainly we cannot draw it at a given

point just because at that point communication with or interpretation of animal feeling becomes peculiarly difficult. The likelihood of things being in certain places is not proportional to the ease of our finding them there. And again, such structures as sense organs and brain are only specialized devices for achieving results which in cruder fashion are attained in the nerveless protozoa; namely, response to environmental conditions and unity of action. What fact opposes the hypothesis that wherever there is such response and such unity there is some form of experience which differs from the human form in complexity and versatility, in a manner and to a degree indicated by the differences in structure and behavior? And who is to show us that the protozoa are the simplest of responsive and organized individuals? Indeed, by what criteria could we identify as such an "individual" which did not act individually with respect to its environment? It is just because a piece of wood or metal lacks significant individuality of action that we regard it as a mere swarm of molecules; and just in so far as the latter lack this individuality they are mere swarms of atoms. As for electrons, they appear to have rather less distinctness than atoms, yet not to lack it altogether.

Thus the empirical facts suggest, far more clearly than in Leibniz's day, the hypothesis which he proposed of a series of individualities with varying degrees of complexity and perfection of organization. To impute experience of corresponding richness or its opposite to these individualities is something more than the arbitrary use of an analogy. For, as Leibniz was the first to clearly see and state, the merely "physical" properties of a thing are exhaustively analyzable as patterns or arrangements, so that to add qualities of experience, shades and nuances of sentience, to our concept of the thing is simply to provide the only possible answer to the question, what is it that is arranged or patterned. To this question of quality we return later. At present our problem is the possibility of a direct verification of the feelings of individuals other than oneself. This problem is set in a new light by the panpsychic conception just sketched. For the individuals most directly and vividly present to us are

those forming certain parts of our own bodies, especially the nerve cells. The pain which I feel in my finger is my pain. Yet I can have varying degrees of identification with the pain, can contrast it as content "there" in the finger with myself as spectator "here," i.e., vaguely in the head and chest region. Panpsychism regards this as the phenomenon declared by positivism to be impossible, the phenomenon of a relativity of self-hood such that the sharing of the same feeling by different individuals is a given fact. The pain is not mine absolutely and simpliciter, but *to a certain degree*, and with reference to a not-myself to which it also in some degree belongs. Physiology suggests that this not-myself is a set of nerve cells. The individual psychology of the human person may be at the same time the crowd psychology of the human nerve cells. To be sure, the crowd of nerve cells is given indistinctly, presenting to observation a mass effect, in which individual outlines are lost. But the effect is vivid, and the spatial contrast of there and here, of self and content is no less so. In panpsychist ontology "there" and "here" can mean only self and other-self.

But there are other aspects of the same general principle. In memory we are at once our present and our past selves. These are the same self relatively, but relatively they are also different sentient individualities. Hence memory is direct vertification of the hypothesis of different individuals with more or less similar feelings.

The mind-body instance of direct participation in others' feelings constitutes an independent check upon the validity of the argument for lower animal feelings from continuity and analogy. For supposing it turns out, as seems probable, that the essential physiological condition of human pain-sensations is some degree of damage to certain cells, or at least, of hindrance to their normal activity. Then the argument from analogy would infer that such cells would experience this to them immediately deleterious situation as suffering. The argument from direct participation—which, be it, noted was above held to be given as such, even if not in a crudely obvious manner—leads to the same conclusion. The same double approach is possible with all sensations. Red is a

"warm" feeling; it involves a certain emotional set of the organisms.[3] If it eventuates that the cells most concerned in the sensing of red exhibit, by analogical evidence, a similar emotional set, then again panpsychism will receive empirical verification, at least as far as the gamut of animal forms is concerned.

Thus it is far from self-evident that the language of data is a merely private language. It may apply intersubjectively to all things. An important point here is that it need not be true that while physical characters are intersensory, common to all the senses, the qualities of sensation, such as redness, are, until translated into physical equivalents, peculiar to each sense. There is a mass of evidence that sense qualities have dimensions common to all the senses.[4] Brightness and warmth are two such dimensions. To measure the brightness of sounds is exactly as easy as to measure that of colors. For the physiological identity of sound and color brightness there is also evidence. But this topic cannot be further developed here.

It is time to consider the second argument for physicalism, which is that the language of experience is equivalent to a part of the language of space-time, namely, to that part which describes events in the experient's body. Panpsychism has two comments here. The first is that, since every individual is to be considered, for metaphysical reasons, in the interest of the unity of science, and with some empirical warrant, as in this sense a "body," the language of experience becomes coëxtensive with the physical language. The second is that the "equivalence" of statements in the two languages is in one sense an exaggeration, and in another sense does not go far enough. It is an exaggeration in the sense that the physical language, as already remarked, is a language of structures, treated as independent of qualities, whereas experience is an affair of structures as constituted by relations of qualities. The latter are defined, as Peirce has taught us, by their monadic character. They are structures of the first or, if you prefer, of the zero degree. It follows that the

[3] See *Psychological Abstracts*, VI (1932), 2084.

[4] Grounds for holding these qualities also *intersubjective* are given by Donald Williams, *Psychol. Rev.*, 41 (1934), pp. 472 ff.

idea of quality is statable in structural language, and hence does not fall outside the realm of the logically mentionable, as positivists sometimes seem, though hardly consistently, to suggest. But the first degree of structure has as such no explicit physicalist expression. On the other hand all degrees of structure are in principle experientially expressible. This suggests that the two languages are not equal for the opposite reason to that given by the physicalist (his reason being that the experiential language is a part and the other language the whole). The true distinction is that, although both languages apply to all individuals, and in this sense are coextensive, the physical language states only an aspect of each individual, whereas the psychical language states all aspects. For behavior can be viewed as changes in one experience as participated in, thereby involving changes in, another experience. The physical or outer side of things is simply the inner side as entering into the nature of other things. It is a relational pattern involved in the social character of existence, as Whitehead has shown in detail (See *Process*; also *Adventures*, Bk. III). Thus the duality of material and immaterial is not a duality of two different natures, nor an ultimate difference even of two languages, since comparative psychology will eventually weld them into one, but is only the fact that individuals are sentient at once of themselves and of each other. There is no mystery in this, unless there is mystery in the fact that being is neither mere unity nor unrelated or absolute plurality.

From another point of view the relation between the languages is more intimate than positivists allow. For the equivalence which they speak of is empirical, not a priori. It involves mutual implication only by reference to psychophysical laws, which have to be empirically discovered. But is it merely a contingent fact that the quality of certain sensations appears together with a certain structure of physical behavior? The direct experience seems to be rather of a mutual aesthetic relevance of quality and structure. Indeed, structure itself seems to have a kind of unitary character, i.e., quality (Peirce's Firstness of Secondness and Thirdness). And there is in the quality of brightness (or, the same thing, shrillness) a certain tenseness well-known to art,

and seeming to unite quality and behavior patterns inseparably. "Warmth" also seems both qualitative and behavioristic, as does pain or pleasantness. The relevance of this consideration, tenuous as it may seem, to the argument by analogy to the qualities of other minds is obvious. Positivists assume a doctrine of external relations between qualities and structures which is really metaphysical, and, I believe, mistaken. A certain relative externality does hold. But absolute externality may be like so many other absolutes, a mere limit of thought to which actuality does not attain. Waiving metaphysical reasons for this view, I remark only that the purely irrational or miraculous character necessarily attaching to the distribution of qualities which were absolutely independent of their contexts renders the supposition of such independence repugnant to that faith in the intelligibility of things through their contexts from which scientific successes derive.

As a final example of the more fundamental status of the psychical language, the bearing of panpsychism on the problem of induction may be mentioned. Positivism leaves the possibility of prediction a pragmatic postulate,[5] as though human needs had power to determine cosmic events. But panpsychism puts the question of cosmic order in a less mysterious light. For it holds that the human need for order is nothing peculiar in nature, but only a special case of a universal law, the aesthetic law of unity in variety. All individuals desire such unity, including the world as an individual, and this desire is the future itself as related to the present, that is, as future. The world has a future only because it "dreams of things to come," this dreaming, or anticipating, or planning, being futurity as we experience or can imagine it. The aesthetic law of unity in variety—a priori demonstrable not empirical—guarantees that the future shall involve neither pure determinism, for that would be absolute monotony, aesthetically intolerable, nor pure chaos, equally intolerable, but relative determinism, like that in Bach's music.

It all comes to this. Experience discloses certain variables or

[5] See H. Feigl, "The Logical Character of the Principle of Induction," This Journal, 1 (1934), 20–29.

dimensions of differentiation. Such are space, time, intensity, the dimensions of the color solid. The questions confronting the whole of science are these: (1) can all the qualities of human experience be described in terms of such potentially continuous variables or dimensions,[6] or must we also assume non-relative differences, such as may be construed neither as that between different values along one dimension nor as that between two dimensions in the same intersecting system of dimensions; (2) are the qualities of the non-human parts of the universe to be conceived as "values," perhaps quite different values, of the same variables as are represented by qualities given to us, or at least of variables falling upon the same intersecting system, or must we suppose that non-human things possess characters that are quite outside the system of variability which experience itself discloses? The second alternative seems in both cases meaningless, for "characters," or any other general concept can be defined only by reference to the system spoken of. Most thinkers agree in regarding space and time as structural variables common to man and other entities; panpsychism is the doctrine that there must also be common *qualitative* dimensions. (The proof is given in Peirce's dialectic of First, Second, Third.) Whether these common structural and qualitative dimensions are called psychic is merely a matter of verbal taste. The point is simply whether there is a common ultimate system of possible differences in terms of which the world may be indefinitely understood. So long as the internal qualities of experience are so carelessly treated as they still are in psychology and philosophy the problem must remain rather indefinite. But the chief danger of positivism is that in excluding questions which it is unable at the moment to define meaningfully it will really "block the path of inquiry," which is a still greater sin against logic than using words without clear meaning. The issue between positivism and such a metaphysics as that of Whitehead (the kind positivism ought to be

[6] See my book, *The Philosophy and Psychology of Sensation* (The University of Chicago Press). Also C. S. Peirce, *Chance, Love, and Logic*, p. 221; or in *The Monist*, Vol. II, p. 547 (to be republished in Volume VI of *The Collected Papers of Charles Peirce*, Harvard University Press).

considering, instead of systems produced by thinkers not trained in science) is partly concerned with certain very real methodological deficiencies in this metaphysics as now presented (I see, for instance, no possibility of a valid grounding of the doctrine of "eternal objects" as this doctrine has hitherto been stated); but it is also, and perhaps in even greater measure, a question of whether or not the path of inquiry is to be kept open by employing hypotheses that do not set limits in advance to the intelligibility of the world as we experience it, or to the unifiability of the sciences. Physics can absorb psychology without intolerable paradox and make-believe only if it be construed as the behavioristic side of a universal comparative psychology by virtue of which even differences of feeling-quality become indefinitely public as well as private facts.

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Stability in Causal Systems

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SECTION I. WHAT IS A CAUSAL SYSTEM?

THE general concept of a causal system has been basic in scientific thought. It may be formulated as follows. The system in question possesses certain measurable attributes such as those of dimensions, temperature, and so forth. In the case of a causal system, it is affirmed that the subsequent development of the system from a known initial condition—that is, a condition in which the measurable variables have known values—is uniquely determined by these values. More definitely, the value of these same variables after any interval of time depends only on the initial values of the variables and the time interval which has elapsed.

In case such a set of variables is not known, the system is more or less indeterminate and exact predictions cannot be made.

Now it is evident that if the laws of development of the system are fully known, then, in particular, the rates of change of the variables involved depend only on the initial values of these variables. When this statement is formulated in mathematical terms, it takes the form of a set of differential equations, as many in number as there are variables. For example, if we consider the fall of a body in a vacuum at the surface of the earth, then the laws in question can be stated as follows: (1) The rate of change of distance fallen with time is the velocity; (2) The rate of

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change of velocity is 32 (in feet and seconds). In this case the two variables which determine the "state" of the body are its distance above sea level, say, and its velocity of fall at the instant considered.

A classic instance of a set of differential equations of such a system is that for bodies moving under the influence of gravitation. The differential equations in this case are merely the symbolic statements of Newton's Law of Gravitation.

The history of science shows that in general an attempt has always been made to embody the laws in the form of differential equations. These laws can usually be determined by experiment, and once they are known, the whole theory of the physical systems is mathematically determinable.

In the cases just referred to there are only a finite number of measurable variables, and the time is thought of as a continuously varying quantity. It is possible to generalize such a system in two different ways. In the first place, we may introduce systems in which an infinite number of variables are involved. Thus the deformation of an elastic solid is only known when the positions of all of its infinitely many points are known. Again, we might suppose that the time, instead of increasing continuously, increases by quanta. Although discrete time has not yet been employed in physical speculation to any considerable extent, it would seem, from a purely philosophical point of view, to be just as satisfactory as continually flowing time. In this case a system of difference equations would take the place of the system of differential equations.

In the practical calculation with actual causal systems, only a limited degree of accuracy is sought, since the laws of the system are at most an idealization of the actual laws, and the isolation of the system from other systems, which is always postulated, can never be more than imperfectly realized. In certain instances, however, such as that of the solar system, the approximation of the actual system by the ideal one is so close that the differences over long intervals of time between actual observations and the results of computation by theory are almost negligible.

Nevertheless the speculative possibility, that, by increasing the number of variables, an indefinitely close approximation to actuality might be obtained, forces itself on one's attention. In addition, the idealized system raises questions of the utmost mathematical interest. In the present paper, we discuss primarily these idealized systems.

We have seen essentially that the mathematical definition of a causal system can be made very exact. Namely it is a system, the variable attributes of which satisfy a system of differential (or difference) equations of certain well defined types. Likewise, the intuitive concept of causality is very familiar. Nevertheless, from a physical and philosophical point of view we take the position that in the last analysis the distinction between causality and non-causality may be purely conventional. This is because it is possible to rationalize any random sequence of events. Thus, for example, if a certain variable y is observed to take on the values f (x), where f (x) is a function of the time x measured from a certain fixed instant and if we assume for simplicity that f has a continuous derivative f′ (x), we may take the following system of differential equations as expressing the "law" of development of the system:

$$\frac{dx}{dt} = 1, \qquad \frac{dy}{dt} = f'(x).$$

Here t measures the time from an arbitrary instant. Of course this system "determines" exactly the observed sequence of events under the initial conditions $x = 0$, $y = f(0)$, for $t = 0$. Under the initial conditions, $x = 0$, $y = a \neq f(0)$, for $t = 0$, the system would determine what *might* have happened and it would be impossible without further knowledge either to deny or to affirm that under these initial conditions the variable y really would not have taken on the values $a + f(t) - f(0)$. Thus our law is a valid one in the sense that it gives an accurate description of a physical phenomenon. The only possible objections are that this law is of no practical use and that it is not aesthetically satisfying. The whole question can be summarized in the query: What is the essential philosophical difference between "post hoc"

and "propter hoc?" Hume of the eighteenth century was the first to point out the fictitious nature of the concept of causality and many modern philosophers hold to his idea.

SECTION 2. PHASE SPACE

A very important mathematical instrument for the study of causal systems is afforded by the representation of the system in what is called "phase space." If there are n measurable variables of the system, we use n-dimensional space. A point then represents a particular state of the system; for just as position in ordinary space is specified by three numbers (the distances from three fixed mutually perpendicular planes, for instance), so position in the n-dimensional phase space is specified by means of n numbers.

Likewise in analogy with three dimensional space it is possible to define the "distance" between two "points" $(x_1, \ldots, x_n)$ and $(y_1, \ldots, y_n)$ as equal to

$$((x_1 - y_1)^2 + \ldots + (x_n - y_n)^2)^{\frac{1}{2}}.$$

A "curve" is defined as the collection of "points" satisfying a system of equations of the type $x_1 = f_1(\tau), \ldots, x_n = f_n(\tau)$ where the f's are continuous functions of a parameter τ defined on a suitable interval. In case these functions are differentiable, we may also define the "direction" of the curve at the "point," $x_1 = f_1(\bar{\tau}), \ldots, x_n = f_n(\bar{\tau})$, as being specified by any set of numbers proportional to the derivatives, $dx/d\tau, \ldots, dx_n/d\tau$, evaluated at $\tau = \bar{\tau}$. These definitions and other similar ones allow us to take over familiar concepts of ordinary 3-dimensional space into the more abstract n-dimensional space. It is found that many of the properties of ordinary space hold also for n-dimensional space. In discussing n-dimensional phase space we shall therefore frequently use the ordinary language of three dimensional space without further comment. The very fact that it is possible to use this familiar language is an instrument of great heuristic power in many mathematical researches.

Suppose now that a point in phase space is taken as the initial state of the system. In accordance with the differential equations

this point will move in a definite direction with a definite velocity, and will describe a curve in the phase space. Similarly every other point will describe a curve. In this way a fluid motion of the phase space is obtained. In this fluid motion all the points move simultaneously in the way prescribed by the differential equations. The direction and magnitude of velocity at each point is always the same. For this reason, we see that the fluid flow so obtained is a steady flow. By means of an understanding of this fluid flow as a whole, we can arrive at an understanding of all the possible types of motion of the given causal system.

This kind of geometrical representation of causal systems was first used fully by the great Henri Poincaré.

An immediate classification of causal systems into closed and open systems is possible on the basis of this idea. If the fluid flow takes place in a finite part of the phase space, then we may speak of the system as "closed." In the contrary case the system is termed "open."

The very definition of a causal system makes it clear that if once the system recurs exactly to its initial position, it will repeat its course of development periodically. In this case, the point in phase space will describe a closed curve and the corresponding motion is called "periodic." A still simpler possibility is that in which the system is at rest. This means that the measurable variables have such values that the rates of change demanded by the differential equations all reduce to zero. For equilibrium, the corresponding curve in phase space degenerates to a single point.

SECTION 3. NON-RECURRENT SYSTEMS

A causal system will be said to be non-recurrent in case there is not a general tendency for the system ultimately to return to the near vicinity of an arbitrary initial state. If we make use of the concept of the fluid motion in phase space, this means that an arbitrary small volume or molecule of the fluid in general never returns to its initial position. Thus, the molecule traces out a tube which never overlaps itself and it is apparent that the same tube, even if extended indefinitely into the past, cannot overlap.

It is easy to give examples of such systems. For instance, consider a pendulum in vibration. The amplitude of its oscillations will decrease, due to friction, and the pendulum will tend toward the vertical position of equilibrium. For such a simple system it is evidently impossible that there should ever be recurrence to the same position and velocity, inasmuch as the total energy of the pendulum (potential and kinetic) constantly diminishes. A second example is afforded by a pendulum clock in which it is supposed that the main spring is possessed of infinite potential energy so that the clock will keep going indefinitely. In this case, if the pendulum is displaced to any position beyond its normal amplitude of oscillation and let go, the vibrations will steadily diminish in amplitude toward the limiting permanent amplitude. In such a motion then, there will not be recurrence. If the pendulum is displaced from the vertical by less than this limiting amplitude, the clock will not go and the vibrations of the pendulum will diminish toward zero. Thus, in this case also, the motion is not recurrent. If, however, the pendulum be drawn to one side to precisely the critical amplitude, the motion will repeat indefinitely and in this special case there will be not only recurrence but actual periodicity.

As a third case of less specific type, we may consider a particle moving in a line according to any law of force whatsoever which permits of only one position of equilibrium. It may then be shown that the motion of the particle, if not periodic, is either (1) of diminishing amplitude of swing about the position of equilibrium, tending toward a limiting periodic motion or toward the equilibrium position, or (2) a motion of increasing amplitude of swing about the position of equilibrium, tending toward a limiting periodic motion, or (3) a motion such that either the distance from the equilibrium point or the velocity, or both are not bounded. This last alternative is ruled out in the case of a *closed* system.

The general characteristics of closed non-recurrent systems are the following: Such a system possesses certain "central motions" which all other motions tend to approach. The general motion, if not central, may be characterized as either one of uniform

approach to these central motions or, exceptionally, of successive approaches to and recession from these central motions at longer and longer intervals of time. In the cases specified above, the central motions are the periodic motions and the equilibrium states. All dissipative systems—that is, systems in which the total energy constantly diminishes—form non-recurrent systems in which the central motions are merely certain equilibrium states.

It is a very interesting fact that the central motions are necessarily periodic or recurrent in type. For this reason, it is seen that the study of non-recurrent systems leads inevitably to that of recurrent systems.

SECTION 4. RECURRENT SYSTEMS

In the case of recurrent systems any molecule in phase space will ultimately overlap itself. This means that, although the system may not exactly repeat itself starting from an arbitrary initial state, yet in general it will return to the vicinity of its initial state and nearly repeat its motion during a long interval of time. Poincaré said that a system of this type possessed stability in the sense of Poisson. This use of the word "stability" is, however, unfortunate. We wish to reserve for this word another meaning and so we shall always use "recurrent" to specify systems stable in the sense of Poisson.

In order that a closed system be recurrent it is sufficient that the volume of a molecule in phase space does not change. We can see this in the following way. A small molecule initially at time t_0 in a certain position in phase space will eventually have moved entirely out of this position at some time $t_0 + \tau$—at least provided that the molecule is sufficiently small and contains no equilibrium states. Now consider the position of the molecule at times t_0, $t_0 + \tau$, $t_0 + 2\tau$, $t_0 + 3\tau$, ... All of these successive positions cannot be distinct, since the total volume is finite in the case of a closed system. Therefore there must be an overlapping of these different positions of the molecule and this leads by easy reasoning to a proof that the system is recurrent.

SECTION 5. VARIATIONAL SYSTEMS

The type of causal system which we are now about to consider briefly has been of central interest in dynamical theory. We call these systems "variational" for reasons which are rather hard to specify in non-technical terms. It was Maupertius who, for theological reasons, first held that the laws for any dynamical system must involve the utmost economy of effort. When this "minimum principle" is put into mathematical form by use of the calculus of variations it leads to certain types of differential equations. We call such systems variational because they are thus deduced.

It is a remarkable fact that, by means of such variational principles, and the concepts of kinetic and potential energy, it is possible for the mathematician to write down in a perfectly automatic manner the complicated equations required in any particular case.

The physical importance of the concept of energy (kinetic and potential) need hardly be stressed, but we will at least mention two examples. Imagine an incompressible fluid at rest in a rigid container, which we will suppose in the form of an ellipsoid. Suppose now, by means of a sudden impulse, the ellipsoid is set in motion. How will the fluid move? The answer is merely that it will move in that particular manner geometrically compatible with the motion of the shell which contains it, which requires the *least* kinetic energy. Again, suppose that we have a circular disc of an elastic substance which is displaced from its position of equilibrium in a prescribed manner by means of pressure on its surfaces. It will move then to that position for which the potential energy of displacement is least.

The classical form in which the variational systems have been embodied are the equations of Lagrange and Hamilton. The notable mathematical power thereby obtained for dealing with these systems has led to a certain amount of mysticism concerning the use of variational principles, so that physicists generally attempt to express all physical laws in variational form. In this connection a quotation may be made from the American

mathematician Benjamin Peirce's book *Ideality in the Physical Sciences* (page 76) written some fifty years ago:

"An equation, first written by the illustrious Lagrange, consisting of only six symbols, $\Sigma p\delta q = 0$, is the universal type by which every problem of mechanics is subjected to analytic discussion. All that remains is for the mathematician to pass the equation through his ideal alembics, and give it the just interpretation. All physical forces and phenomena, possible as well as actual, ideal as well as real, are condensed into this formula and subject to its magical power. It embraces the complete history of the material universe from the beginning to the end."

Perhaps the most notable case of a variational system is that afforded by the Solar System under Newton's Law of Gravitation. However, it is not too much to say that all purely dynamical systems in which there is no dissipation of energy fall under this variational type.

What then is the general characterization of variational systems? In the first place, the volume of a molecule in phase space remains unchanged. Hence, according to what was said in the preceding section, a variational system is necessarily recurrent if it is closed. A simple illustration of this fact is afforded by two particles which attract one another according to Newton's law. If the kinetic energy is not too great, they will then describe ellipses about their center of gravity as focus, so that all motions are periodic and therefore recurrent. It should be noted in this connection, however, that the same system is not closed for large values of the energy constant. In this case the orbits are hyperbolas or parabolas and there is no recurrency.

The motion of a closed variational system may be roughly characterized by saying that it is either periodic, or uniformly recurrent, or approaches and recesses from a set of uniformly recurrent motions at longer and longer intervals of time. Here the phrase "uniformly recurrent motion" is applied to motions which, like periodic motions, cover their complete states to within a small error in all sufficiently large intervals of time.

In this connection the "ergodic theorem" may be mentioned as a recently proved theorem which informs us that *nearly all the*

motions of variational systems exhibit uniform tendencies with respect to any specified average. We describe this in more technical language as follows: Let P_t denote the point in phase space representing the state of the motion at time t and let f (P) denote any function integrable over the phase space. Then

$$\lim_{S+T=\infty} \int_{-S}^{+T} \frac{f(P_t)dt}{S+T}$$

exists, except possibly for motions the points of whose trajectories form a point set of Lebesgue measure zero. We shall discuss the significance of this important theorem in a later section.

All the above mentioned characteristics of closed variational systems are enjoyed in common with all other closed systems which have the property that the volume of a molecule in phase space does not change. Variational systems, however, have certain further "integral invariants" which serve to distinguish them from these more general systems. One important consequence of this fact is the existence in general of certain periodic motions of "stable type." These periodic motions will be discussed in a later section.

SECTION 6. THE GENERAL CONCEPT OF STABILITY

We come now to the question of stability. The fundamental fact to observe here is that this concept is not in itself a definite one but is interpreted according to the question under consideration. We may, however, approach it in the following manner. Certain parts of phase space are regarded as "regions of stability," the other regions as regions of instability. A motion then will be said to be "stable" if, after the instant in question, its curve of motion forever lies in the stable part of phase space. Thus, if we take an idealized problem of the Earth, Sun and Moon in which these bodies are thought of as mass particles moving in the same plane according to the Law of Newton, then we might define the stable regions of phase space as those in which the distances between the Earth and Moon and between the Earth and Sun lie between certain specified limits. The motion of the three bodies is then stable if these limits are never exceeded, thus

eliminating the possibility of collision of the three bodies or of indefinite recession from one another. It is interesting to remark in this connection that the American astronomer G. W. Hill showed that there would actually be stability in this sense in case the Moon is assumed not to disturb the motion of the Earth and Sun, at least to the extent that the Moon could not ever collide with the Sun or recede indefinitely from the Earth and Sun. In Hill's work the Earth and Sun are supposed to revolve in circular orbits.

It will be seen that this definition of stability is so general that it can hardly be called a definition at all. Nevertheless any less elastic definition will fail. The definition of thermodynamic (stable) equilibrium given by LeChatelier, as that state in which a slight disturbance produces reaction which tends to restore the system to its initial condition, has at first a pleasing appearance of definiteness; but, when actual application is made, it turns out to be incomplete. For what is a slight disturbance?

SECTION 7. STABLE EQUILIBRIUM

The region of stability of phase space may be taken to be a small region containing within its interior an equilibrium point. The equilibrium point is called stable if, no matter how small this region of stability may have been chosen, any motion initially sufficiently near the equilibrium point will remain in the region of stability indefinitely.

There are a large number of familiar instances of stable equilibrium, both for recurrent and non-recurrent systems.

Perhaps the simplest example, in the case of a variational system, is that of a simple pendulum without friction, the arm of which is supposed to be rigid. In this system we have two positions of equilibrium, one corresponding to each of the two positions in which the arm is vertical. In case the arm of the pendulum is hanging vertically downward we have stable equilibrium. If the pendulum is slightly disturbed from this position it will oscillate forever in the immediate vicinity of equilibrium. On the other hand, the other position of equilibrium in which the arm is delicately balanced in an upward position is obviously

unstable. The slightest disturbance will cause the system to deviate more and more from the equilibrium position. It will be observed in this example that the points of stable equilibrium correspond to the points where the potential energy is a minimum. That this state of affairs is true in general is a familiar fact first noted by Lagrange and proved by Dirichlet. Other critical points of the potential energy, such as points where the potential energy is a maximum, also yield equilibrium points, but they are always unstable.

To obtain a simple example for the case of a non-recurrent system, we have only to add friction in the example just considered. We shall, however, discuss two more complicated examples.

In the first place, consider an elastic structure of any sort, which is to be regarded as a dissipative system in which any transient motion of vibration rapidly approaches equilibrium. If this structure be taken to be the steel framework of a building, the position of equilibrium will depend on the loading of the framework. This loading will give rise to certain stresses and in case these stresses do not exceed a certain amount—corresponding to the limits of elasticity—the structure will be stable. On the other hand there are obviously certain critical conditions when the stresses are just equal to the elastic limits, when a slight displacement of the system will cause the building to become permanently deformed if it does not actually cause a total collapse. This corresponds to an unstable equilibrium. In this example we are, of course, really considering not one system only but infinitely many at the same time corresponding to the different possible types of loading.

In the second place we may consider a physico-chemical system formed by various solids, liquids and gases, in physical juxtaposition at a certain temperature and pressure. Here also after a certain time a practically steady state of equilibrium is reached. If then an arbitrary disturbance is introduced, either by the addition of small amounts of certain substances or modification of temperature and pressure or otherwise, certain small resultant modifications in the position of equilibrium will take place, at

least, unless certain critical conditions exist in the beginning. Here we have again the case of a set of central motions which degenerate to points of equilibrium of phase space. The small modifications imposed on the system can be looked upon as changes in certain variables connected with the system. When these are changed there is in general a small change in the position of equilibrium, but in certain exceptional cases a large change may be induced. For example, if water at atmospheric pressure is at a temperature above 0° Centigrade, it can be slightly cooled and there is no other change, whereas if the temperature is lowered from exactly 0°, the water freezes and there is a large change of state. In this latter case there are several points of equilibrium in phase space which are generally maintained under slight disturbances; but under certain critical conditions a change from one type to another may take place.

In this analysis of the stability of equilibrium of non-recurrent systems the transient states which ensue upon disturbance have been ignored. For the sake of a complete understanding of the phenomena, it is necessary to consider the system as in motion. Thus, for instance, in considering the stability of buildings in earthquakes, it will be necessary to consider the effect of oscillatory disturbances. Similarly for a deeper understanding of physico-chemical systems, it is necessary to determine the laws governing the transient states. These laws are those embodied in the science of physical chemistry.

SECTION 8. STABILITY OF PERIODIC MOTIONS IN VARIATIONAL SYSTEMS. THE BILLIARD BALL PROBLEM

There is another highly interesting type of stability which arises in the case of variational systems. In this case the motion does not remain near equilibrium but remains near a particular periodic motion, at least for extremely long intervals of time. More precisely, the nearby motions oscillate in the "near" neighborhood of the periodic motion for extraordinarily "long" periods of time in the sense that it is mathematically possible to approximate to the disturbed motion during these long time intervals by means of certain periodic, or "almost" periodic,

expressions. It is not possible to give here the technical meaning of the very indefinite words "long," "near" and "almost." Suffice it to say that the computation of the motion can be carried on indefinitely by means of such expressions. A very important example of this state of affairs is found in the solar system. For example, the actual motion of the dynamical system consisting of the Sun, Earth and Moon is not exactly periodic. Nevertheless it is so near a known periodic motion of stable type that the above computation can be carried out to a high degree of accuracy. This process leads to lunar tables by which astronomers can predict eclipses many centuries in advance, a fact that is a never ending source of wonder to laymen.

An extremely important unsolved problem, namely the famous "problem of stability," is to determine whether or not these periodic motions of "stable type" are stable in a rigorous sense. That is, do the nearby motions remain in the neighborhood of the periodic motion for *all* time, instead of merely for an "extraordinarily long" time? This question is of such importance that we wish to elaborate the whole theory of periodic motions with regard to an example, which, although much simpler than the solar system, presents all the essential features of the problem.

Consider an idealized billiard table the boundary of which is a convex closed curve without corners. The billiard ball is supposed to move without friction, rebounding from the sides at the same angle as that at which it strikes.

In case the billiard table is elliptical we can give a complete description of the motion based on an elementary theorem of analytical geometry: Two straight lines intersecting at a point on an ellipse and making equal angles with the normal at this point are necessarily each tangent to some conic which is confocal with the given ellipse. The path of the billiard ball is then a broken line with its vertices on the given ellipse and with each of its segments (or these segments extended) tangent to another conic, which may be either an ellipse or a hyperbola. Figure 1 indicates the motion in case the confocal conic is a hyperbola. Figure 2 indicates the motion in case the confocal conic is another ellipse. In either case the shaded area represents a part of the

billiard table which is obviously inaccessible to the ball during its entire motion.

There are also certain limiting cases. For example the billiard ball may describe periodic motions along either the major or minor axes or it may roll around the boundary of the elliptical table.

The last of these motions is obviously stable. That is, if the billiard ball is set in motion near the boundary in a direction very nearly parallel to the tangent at the nearest point on the

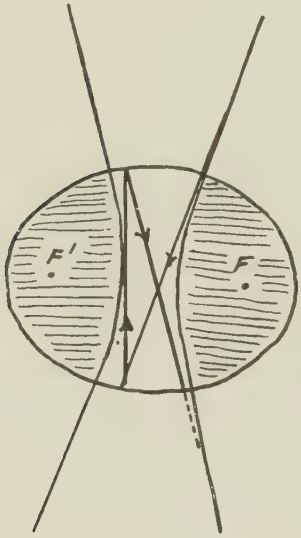

Fig. 1

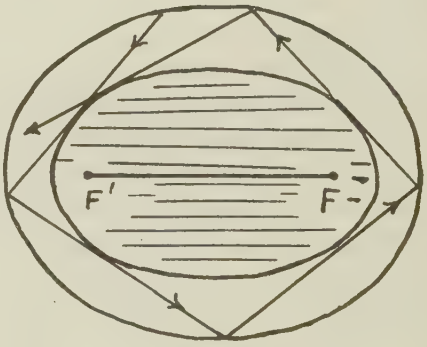

Fig. 2

boundary, the region accessible to the ball is a very thin region bounded by the edge of the table and another confocal ellipse almost as large.

Likewise the periodic motion along the minor axis is stable: for the only part of the table accessible to the billiard ball is the region lying between the two branches of a confocal hyperbola, and these two branches will be very near to each other and on opposite sides of the minor axis, if the billiard ball is originally started near the center and in a direction differing but slightly from the direction of the minor axis.

Similarly it is easily seen that the periodic motion corresponding to the major axis is unstable. In this case the confocal conic is a very narrow ellipse stretched like a rubber band (so to speak) around the two foci F and F′. If the billiard ball happens to pass through a focus (in any direction other than exactly the direction of the major axis), this confocal ellipse degenerates to the line segment FF′ and the billiard ball will continually pass from one focus to the other bouncing on the edge of the table between times.

Thus, in the case of an elliptic boundary we can classify the three most obvious periodic motions as being of either stable or unstable type. Furthermore, we know that those of stable type are rigorously stable in the sense that nearby motions remain in the vicinity of the periodic motion for all time. We are enabled to make these assertions on account of the special geometrical properties of the ellipse which thus render this particular dynamical system "integrable." The problem of stability is thus concerned primarily with more general systems as elucidated below.

Let us now consider the more general case where the boundary is any convex oval symmetric with respect to two mutually perpendicular lines, which we may take as the X and Y axes (see fig. 3). Evidently it is possible for the billiard ball to move backward and forward in a periodic motion along either of these axes. We wish to consider the motion if the ball is started at B near the origin on the X axis at an angle θ with the X axis, where θ differs only slightly from a right angle. For simplicity let us assume that O A = 1, that the curve is analytic in the neighborhood of A and that its curvature at A is equal to α. In this case the equation of the curve near A may be written in the form.

$$y = 1 - (1/2)\alpha x^2 + \text{terms of higher order.}$$

The initial state of the motion is specified by the (sensed) distance u of B from the origin and the number $v = \cot \theta$. The billard ball rolls in the direction BC, bounces on the upper part of the curve at C, then travels to the lower side, where it bounces again, and finally comes back to the X axis, which it intersects at

a distance u′ from the origin, with a direction which makes an angle θ' with the X axis. The new state of the motion is specified by the numbers u′ and $v' = \cot \theta'$. The motion of the billiard ball thus defines a transformation T from the initial "point" (u, v) to the point (u′, v′). The point (o, o) corresponds to the initial state of the given periodic motion and T will carry this point into

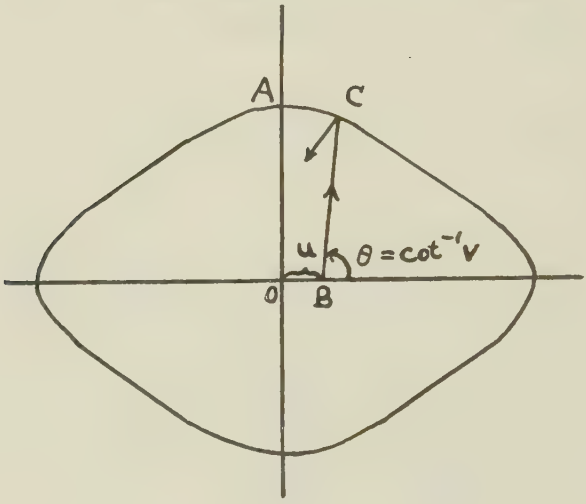

Fig. 3

itself. T may be expressed as follows by means of power series in u and v:

$$u' = au + bv + \cdots$$

$$v' = cu + dv + \cdots .$$

where the dots stand for terms of higher degree than the first in u, v, while a, b, c, d may be readily calculated in terms of α.[2] An interesting fact is that the determinant ad − bc is exactly equal to 1, a result which could have been predicted from the mere fact that the billiard ball problem is of variational type.

We are primarily interested in the effect of repeated application of the transformation T. If a region surrounding the origin in

[2] It turns out in fact that $a = d = 8\alpha^2 - 8\alpha + 1$, $b = 8\alpha^2 - 12\alpha + 4$, $c = 8\alpha^2 - 4\alpha$.

the (u, v) space exists such that points arbitrarily near the origin leave the region upon repeated application of T, the origin is called an unstable invariant point and the corresponding periodic motion of the billiard ball is likewise obviously unstable. In the contrary case, the invariant point and the periodic motion are stable.

In order to examine the behavior of T it is convenient to introduce into the (u, v) space new coördinates. There are essentially two cases according as the quadratic equation $\lambda^2 - (a + d)\lambda + (ad - bc) = 0$ has two unequal real roots $\rho > 1$ and ρ^{-1}, or two conjugate imaginary roots $e^{i\varphi}$ and $e^{-i\varphi}$.[3] We shall not consider the relatively rare case of equal roots.

Case I. It follows from algebra that it is possible in the case of real roots to write T in the form,

$$\xi' = \rho\xi + F$$
$$\eta' = \rho^{-1}\eta + G$$

where ξ and η are homogeneous linear functions of u and v; ξ' and η' are the *same* linear functions of u′ and v′, and F and G stand for power series in ξ and η beginning with terms of degree not lower than the second. Since F and G are infinitesimals of at least the second order in the distance to the origin, it seems probable that points on the ξ axis arbitrarily near the origin (but not coinciding with it) will ultimately move out of some sufficiently small preassigned neighborhood of the origin. This is, in fact, the case, although the rigorous proof is too long to be included here. This type of motion is therefore definitely unstable.

Case II. In case the roots are conjugate imaginary, it likewise follows from algebra that T can be written in the form

$$\xi' = \xi \cos\varphi - \eta \sin\varphi + F.$$
$$\eta' = \xi \sin\varphi + \eta \cos\varphi + G.$$

[3] In either case it is obvious that the roots must be reciprocals of each other, since the constant term in the quadratic equation is $ad - bc = 1$.

Here the linear terms represent a rotation of the ξ, η-plane about the origin through an angle φ. The linear terms by themselves form a stable transformation. The problem therefore is to determine the effect of the terms F and G. In case φ is a rational multiple of 2π, it has been proved by Levi-Civita that such a transformation is in general unstable. In case φ is not a rational multiple of 2π, the given periodic motion is said to be of stable type, even though it is not known whether or not it is stable in the rigorous sense. On account of the variational origin of the underlying dynamical problem, it may be proved that by a futher change of variables the above formulas may be simplified in such a manner that the series F and G begin with terms of degree not less than μ, where μ is arbitrarily great. In this case the angle φ depends in a simple manner upon $\xi^2 + \eta^2$ instead of being a constant. One might think that it would be possible to pass to the limit $\mu \to \infty$. Unfortunately this process would involve the use of the famous "formal series" of dynamics that are definitely known not to converge in certain instances.

The roots of the quadratic equation

$$\lambda^2 - (a + d)\lambda + ad - bc \equiv \lambda^2 - 2(8\alpha^2 - 8\alpha + 1)\lambda + 1 = 0$$

turn out to be $8\alpha^2 - 8\alpha + 1 \pm 4(1 - 2\alpha)\sqrt{\alpha(\alpha - 1)}$ and thus the stable type can occur only for $0 < \alpha < 1$, and we certainly have instability for $\alpha > 1$. This checks immediately with the facts already observed for the special case of the elliptical billiard table, where motion along the major axis was found to be unstable and motion along the minor axis stable.

It may well be possible to solve the problem of stability in the negative sense by restricting attention to the few simple facts presented here. Namely, it might be possible to construct an example of a billiard table affording such a periodic motion of stable type which by geometrical methods could be shown to be unstable in the rigorous sense. Such a piece of work would be of extreme scientific importance not only for its own sake but also because of its intimate connection with various questions concerning "transitivity" which we shall touch upon briefly in the next section.

SECTION 9. PROBABILITY, THE ERGODIC THEOREM, TRANSITIVITY AND THEIR RELATION TO PHYSICS

Whether or not the universe itself is one vast causal system is a question open to speculation. Is it theoretically possible, with the aid of an enormously complicated system of differential equations, to determine the whole course of destiny from a complete knowledge of the state of the universe at any given instant? If this is true, we must evidently accept the mechanistic view of life. It is scarcely necessary to say, however, that causal systems are not the only ones considered by modern physicists. Notably in the quantum mechanics,[4] we have the "principle of indeterminacy," and, in atomic theory generally, the theory of probability plays an important rôle. According to these theories the more classical causal theories of physical systems are true only in a statistical sense. Many amateur philosophers have seized upon this principle of indeterminacy to lay the basis for tychism, with the purpose also of treating the problem of the freedom of the will. It must be remarked, however, that this idea receives the serious support of only a minority of either the professional physicists or philosophers; and we ourselves exclude this aspect of the subject from further consideration.

In order to treat any non-causal system on a mathematical basis, it seems necessary to introduce the notions of probability. Such notions involve the assumption that certain sequences of ratios approach limits. For example if a coin is thrown n times and if h(n) denotes the number of times that heads turn up, the usual assumptions of the theory lead to the result that

$$\lim_{n=\infty} \frac{h(n)}{n} = \frac{1}{2}.$$

This result can be approximately verified by experiment. Similarly, the statistical results of the quantum mechanics are attested by experiment, mostly of a spectroscopic nature. The assumption that certain sequences converge, that is, that certain prob-

[4] Cf. P. W. Bridgman, "The New Vision of Science," Harper's Magazine, March 1929, for a popular exposition of the principle of indeterminacy.

abilities exist, is part of the physical law in question. There is no *a priori* reason why such a law should not be wholly adequate.

It is obvious, however, that many problems which we are accustomed to treat by the theory of probability are susceptible, theoretically at least, of a much more exact treatment. For example, it would be possible to predict (in each given instance and not merely statistically) the result of throwing a coin, if we knew exactly how the coin was thrown, all about the initial position and velocity, the effect of the air resistance and other disturbing factors.

Another more important instance is concerned with a closed variational dynamical system. It is conceivable that a person, ignorant of the causal nature of the system or unable to solve the differential equations, might treat the system by using the hypotheses of probability. Let the state of some motion of the system at time t be represented by the point P_t and consider a certain fixed volume V in an isoenergic manifold of the phase space. Let $T(\tau)$ denote the total length of time spent by P_t in V while $|t| < \tau$. Then our hypothetical ignoramus might construct a gambling game based on the probability of P_t being found in V.[5] This probability is evidently given by the limit as $\tau \to \infty$ of the ratio

$$\frac{T(\tau)}{2\tau},$$

if this limit exists. Now our gambler, in order to lay down profitable bets must have a fairly good knowledge of the behavior of this ratio. On account of his use of the hypotheses of probability he assumes without proof that the limit exists and he may then approximately evaluate it by means of a lengthy experiment.

The founders of the kinetic theory of gases were gamblers of this very sort. The theory explains thermodynamical phenomena as the statistical result of a closed dynamical system composed of a large number of interacting molecules. At one point in the

[5] The billiard ball system would furnish an excellent gambling game of this type. The ball could be set in motion in a given direction and bets could be laid as to whether the ball would be in a certain region on the table at the sound of a random gong.

theory, namely in the derivation of the Maxwell-Boltzmann law for the distribution of the velocities of the molecules, it is necessary to assume that

$$\lim_{\tau=\infty} \frac{T(\tau)}{2\tau} = \frac{V}{\Omega}$$

where Ω denotes the total volume of the given isoenergic manifold in the phase space. This assumption was originally made purely on the basis of probability. In recent years, however, it has become almost possible to explain the originally non-causal theory of gases in terms of the underlying causal theory of variational dynamical systems. Namely the ergodic theorem states precisely that the above limit does actually exist for closed variational systems, at least with the exception of a comparatively small number of motions.[6] Furthermore, if the system has a certain further property, called "metrical transitivity," this limit is equal to V/Ω. Whether or not systems generally have this property of metrical transitivity is an important unsolved problem.

It is beyond the scope of this article to give a complete definition of "metrical transitivity." However, a variational system with fixed energy constant is said to be *simply* transitive if there exists at least one motion which comes arbitrarily near every point on the isoenergic manifold. Every metrically transitive system is also simply transitive. Thus in order to show that dynamical systems are generally metrically transitive it is necessary, though not sufficient, to show that they are simply transitive. And in order to establish this result it is again obviously necessary to settle in the negative sense the essentially simpler problem of stability, which we have formulated in the previous section.

It is interesting to note that from a physical point of view the *causal* classical theory of thermodynamics has in back of it a *non-causal* theory, namely the kinetic theory of gases. This latter has in back of it, as we have just seen, the *causal* theory of

[6] The trajectories of these exceptional motions form a point set of Lebesgue measure zero in the phase space.

variational dynamical systems, with the use of which we can prove the existence of certain limits which play the rôle of probabilities in the non-causal kinetic theory. But classical dynamics itself is a mere statistical average of the quantum dynamics, which with its principle of indeterminacy is *non-causal.* We thus have the accompanying diagram, explaining in an obvious

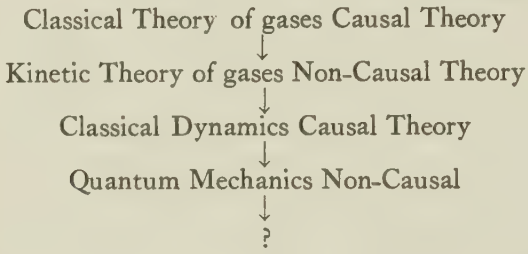
Classical Theory of gases Causal Theory
Kinetic Theory of gases Non-Causal Theory
Classical Dynamics Causal Theory
Quantum Mechanics Non-Causal
?

Fig. 4

manner the relation between these various theories. One is now tempted to ask if there is a "causal" theory in back of the quantum theory.[7] If so, is *it* the fundamental theory of the universe, or is there a still more fundamental non-causal system in back of it? May there not be an infinite number of such theories, alternately causal and non-causal, each theory more "fundamental" than its predecessor? In the present rather unsatisfactory state of the quantum theory (especially the relativistic quantum theory) it is impossible to answer any of these questions.

In conclusion of this section, it is to be observed that the notion of transitivity is the very antithesis of the general concept of stability as expounded in Section 6. The two notions therefore interplay with each other, if only in counteracting senses, and both

[7] Let a_n be the average result of measuring simultaneously, say, an observable α in each of n like systems in the normalized state ψ. The notation of Dirac is here used. Our hypothetical causal theory would therefore have to yield an explanation, based on certain aesthetically satisfying physical "laws" yet to be discovered, of why $\lim_{n=\infty} a_n$ exists and is equal to $\varphi\alpha\psi$. Such a "causal" theory might be of a very abstract nature and the time-like independent variable in the underlying differential equations might have little or nothing to do with ordinary time. In fact it might be in accordance with the theory of relativity in which time and the spacial dimensions are treated almost on an equal footing.

are of supreme importance in the general philosophy of physical systems.

SECTION 10. STABILITY IN BIOLOGICAL SYSTEMS

We wish now to devote a section to the applicability of the foregoing methods to problems concerning life.

For inorganic phenomena no single physical theory can properly be called fundamental although we have seen the interrelations which exist between some of them. For organic phenomena, which are perhaps to be regarded as mere extensions and complications of the inorganic, it is likewise probably possible to build up a vast hierarchy of theories no one of which is entirely adequate but each one of which is fundamental in certain directions. Thus one could hardly expect to treat by exactly the same theory the growth of a simple cell and the growth of the population of the United States. In fact the behavior of the nation as a whole is determined statistically by the behavior of its citizens in much the same way that the behavior of a gas is determined statistically by the action of its molecules. And likewise the individual in its turn can be regarded as a highly complex system consisting of many different tissues each of which is composed of millions of cells.

It is not fully known to what extent these biological sytems can be represented by ideal causal systems. Assuming, however, that such a representation is possible, we wish to examine some of the broad characteristics of the governing differential equations. In order to do this we note that, broadly speaking, we shall obtain systems of three rather distinct natures.

The first type of system would be of use in dealing with any biological system which is looked upon as an individual. And here we are using the word "individual" in the broadest sense of the word, for example, an individual animal, an individual colony of fruit flies, or an individual nation. Such a system will be called an individualistic system.

The second type of system would be of use in dealing with a long succession of individuals. And we also use the phrase "long succession" in its very broadest sense. For example, a long

succession of some very rapidly breeding organism might be exhaustively studied within a year or even within a few days, whereas other long successions might extend throughout the course of history or even longer. We shall call systems of this type evolutionary.

The third type of system deals with the relations of a comparatively small number of individuals. That is, we have here to deal with more than one individual but not so many that it is possible to consider them in their totality as forming a single individual of higher order.

One of the principal characteristics of the individualistic system is the presence of at least one "natural" variable which

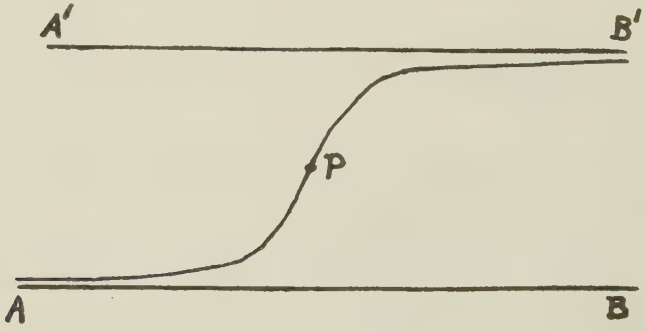

Fig. 5

considered as a function of the time has a very strong tendency to take the very characteristic shape of an absorption curve. This variable, in the case of a human being, might be weight or height, in the case of a nation, it might be the population. In fact it has been found experimentally that many forms of growth phenomena can be described with great accuracy by means of such curves as the logistic or Gompertz curves.[8] Both of these curves are essentially of the same type as that represented in figure 5. The curve is asymptotic to the parallel lines AB (which represents the t-axis) and A'B'. It has one point of inflection

[8] Cf. "The Gompertz curve as a growth curve" by C. P. Winsor. Proc. of the Nat. Ac. of Sciences, vol. 18 (1932), pp. 1–8.

at P and no critical points. The curve has essentially two parameters (other than that corresponding to fixation of the origin of time) namely the slope at P and the distance between the two parallel lines. By modifying these two parameters it is found that the curve can be fitted with great accuracy to a vast variety of statistical data representing growth. For example it has been shown that the growth of the population of the United States has in the past followed such a curve with great accuracy and is supposed to have passed the point of inflection in April of 1914. Likewise the growth of an organism can be represented by such a curve.

It is found experimentally that the biological variables represented by this curve are exceedingly stable. For example, experiments have been made on the increase in number of flies in a large bottle under various conditions of food supply, the volume of the bottle being, of course, kept constant. Disturbances on the system of various sorts were introduced, but the birth and death rates always adjusted themselves so that the deviations from the growth curve were extraordinarily slight. Likewise it is thought that the growth curve for the population of a topologically restricted nation is almost independent of sudden disturbances such as immigration, emigration or the loss of large numbers of the population in epidemics or wars. In the growth of a human being, it is also common knowledge that the characteristic curve of weight, say, against time is only temporarily modified by a slight illness or an orgy of over-eating.

What then can one deduce concerning the nature of the hypothetical individualistic type of causal system? It would seem that such a system is characterized by the presence of two sets of central motions, corresponding to the two asymptotes of the growth curve, and regions of stability in which are embedded the transitional motions which lead from the neighborhood of the "lower" set of central motions to the "upper." It should be remembered, however, that such an idealized system fails exactly to represent the facts at the two ends of the life of the individual, near birth and near death.

In considering a system of evolutionary type, we are con-

cerned with a long succession of life cycles, the laws of heredity and the factors which produce variations in the different generations. It seems almost impossible to make general remarks about a causal system of this type except to say that it is almost certainly of open type. For example the number of generations counted from a certain fixed instant would almost certainly be one of the fundamental variables and this would obviously increase indefinitely.

The theory of the third type of system embraces as a special case Pareto's quasi-mechanistic social theory. Here, as in the individualistic systems, we seem to have a certain type of stability. Pareto, himself, has defined the concept of stability in the social domain essentially as follows (we are quoting L. J. Henderson):[9] "If a small modification different from that which will otherwise occur is imposed upon a system, a reaction will at once appear, tending towards the conditions that would have existed if the modification had not been imposed."

Evidently such a concept of stability is patterned upon or at least is similar to that of non-recurrent and in particular dissipative systems tending towards an equilibrium position after a disturbance. It is possible here to think of the final conditions as merely neighboring rather than as the same, and thus to admit types of permanent slight variations analogous to those in the equilibrium of physico-chemical systems upon alteration of some of the variables.

The reaction referred to is that which must exist to ensure equilibrium; for there are always slight disturbances present, so that any social order which has existed is to be presumed stable on that account.

It is clear that the types of "forces" which impinge upon Society are extremely numerous. If we neglect material factors, a first attempt at their classification may be regarded as that of Pareto who accords a first place to the sentiments of which the "residues" are the manifestations.

[9] Cf. his book: "Pareto's General Sociology, A Physiologist's Interpretation" (Harvard University Press, 1935). Professor Henderson analyzes the notion of equilibrium in the social order as compared to equilibrium in a physico-chemical system.

SECTION 11. STABILITY OF PERSONALITY

We often speak of a stable or integrated personality. Dare one go so far as to use the concept of stability in its relation to personality? Perhaps yes, in the sense that stability be measured by the ability to meet serious crises without important break in character and ideals.

The best dynamical analogue to an unintegrated or unstable personality might be found in a dynamical system of non-recurrent type which possesses at least two distinct central motions so related that a disturbance is sufficient to throw the system more or less frequently from one of these central motions to another. If the strain of the intervening transient states upon the system is so great as to have a destructive effect, the analogy becomes still more complete. Here one type of central motion might be thought of as "higher" energy the other as of "lower" energy. It is the passage from higher to lower which is to be regarded as disruptive since it is here that potential energy is lost.

Despite the vagueness of such concepts of stability for biological systems and personality, there can be no doubt that these enormously complex systems are causal to a certain extent, and this fact makes the use of the idea extremely important for the comprehension of the facts.

SECTION 12. ON NIETZSCHE'S THEORY OF "DIE EWIGE WIEDERKUNFT"

The full extent to which dynamical and other scientific ideas influence philosophical speculation has only been realized recently.

Even as we write there appears an article by Lecornu[10] illustrating typically this fact. The article treats an idea closely related to causal systems which came to Nietzsche in a kind of revelation after reading certain mathematical works of Helmholtz and Riemann. This was his celebrated doctrine of eternal recurrence which reaches its clearest and most poetical formulation in the following quotation from his book, *Also Sprach Zarathustra:*

[10] "Sur le retour éternel," Comptes Rendus, Jan. 21, 1935.

"Siehe, wir wissen, was du lehrst: dass alle Dinge ewig wiederkehren und wir selber mit, und dass wir schon ewige Male dagewesen sind, und alle Dinge mit uns."

His idea was that, after immense periods of time, there would be a return to the present state and thus an infinite number of returns. Lecornu comes to the conclusion, on the basis of modern physical law, that it is only a strange dream and perhaps a first indication of Nietzsche's later madness.

In our opinion this conclusion is an exaggeration of the actual case. If one admits that the laws of the universe *can* be expressed as differential equations involving the $3(n-2)$ variables of which Lecornu speaks, and, if one admits also that these equations define a closed system of recurrent type, such as is found in classical dynamics, then it follows that if there is not exact repetition there will at least be approximate recurrence. In other words, within a very large but finite period of time, our article will again appear on this same subject, in this same journal, read by the same individuals, as far as one may discern, and this will happen indefinitely often.

This assertion of Nietzsche would be exactly realized in case time were discrete and the number of states extremely large but finite. If these states succeeded each other in a causal manner (i.e. if each state, whenever it occurred, were always replaced by one and the same successor), there would evidently be eternal recurrence of the Nietzschean type.

Thus, from a purely mathematical point of view, there is nothing particularly absurd in the conclusion of Nietzsche. The only difficulty is that the system of differential (or difference) equations referred to never can be written down or followed by anyone and, therefore, is to be regarded as meaningless.

CONCLUSION

To sum up, all that stability can mean is that, for the system under consideration, those motions whose curves lie in a certain selected part of phase space from and after a certain instant are by definition called stable, and other motions unstable. Thus

the possible types of stability are infinitely numerous and varied. The most significant types, however, from the standpoint of practical application are the stability of recurrent and non-recurrent systems about positions of equilibrium and in the neighborhood of periodic motions in the case of variational systems. The antithesis of the notion of stability is that of transitivity, the far-reaching importance of which in the kinetic theory of gases and more generally in the philosophy of causal and non-causal systems has been pointed out.

No matter how fascinating the purely mathematical study of causal systems may be, it would seem not to be desirable to take them too seriously from a realistic point of view as applicable to the actual universe. The real purpose of physical speculation is to enable us to calculate only within certain prescribed limits of error and for reasonable intervals of time the behavior of physical systems.

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An Examination of the Quantum Theories. IV

BY

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WHITHER PROGRESS?

WE PAUSE in the process of exposition to note what continuity we may in the progress of the theories of discontinuity we have been discussing. The following trends are rather distinct: (1) the development of more and more general mathematical approaches, (2) a marked broadening of the field of experimental issues, and (3) an increasing attention to analysis of fundamental concepts, the philosophy of physics.

Let us mix optimism and pessimism in our appraisal of progress, since indeterminacy has appeared with regard to the very course of physics itself. The epithet "decadence!" has come not only from the "low-brows," the lowly experimentalists of a decade or two ago, but also from die-hard theoreticians who still miss some ingredients of precious worth of the old theories. Two such ingredients, perhaps, we ought to mention in passing: *determination* and *stability*. No quantum theory really guarantees either of these two seeming desirables. We proceed, however, to details.

MATHEMATICAL PROGRESS

First, what has mathematics to offer toward the further progress of quantum theory, or all physics? Relativists and

quantum theorists have been scouring the mathematical libraries to find some form of mathematics to hang a case on. Some think they have nearly exhausted the possibilities of formulating the most general mathematical mould. An almost necessary bias in the choice of a symbolic algebra has been the requirement that the algebra chosen must be *non-commutative*, since most of the resolutions of paradoxes and predictions of new phenomena have been produced by tools with such an edge.[1] In this field one speaks of "algebra," rather than "geometry," a distinction which may seem significant to some who would see danger in too close an identification of physics and mathematics as may perhaps occur in a "physical geometry," and is less apt to occur in a form of mathematics in which the symbols have traditionally been referred to as "imaginary." There is, however, no reason to imagine that any mathematical physicist really thinks physics to be nothing but mathematics, even when he declares God to be a mathematician. The essential reason for choosing an "algebra" is that it gives a better *fit* as things stand now. It is quite possible that certain theorems in number theory may turn out to be very significant. Topology has not had its proper say yet. There are still numerous possibilities in pure mathematics untested or too difficult to test.

The issue between continuity and discontinuity, so often asserted to reside in the physical situation, should properly be relegated to the underlying mathematics. A philosophic-minded physicist[2] should find no serious trouble with dissecting some well-

[1] R. J. Seeger, Proc. Nat. Acad. Sci. 17,301 (1931) writes for the idem symbol of all symbolic algebras of quantum theory $I \equiv \alpha \{u, v\} \equiv \alpha (uv - vu)$ and a symmetry condition for the independent elements, q and p, namely $uv^2u \equiv vu^2v$. Theories may disagree as to (1) identification of u and v (2) the unique interpretation of q and p, e.g., as operators, matrices, etc., and (3) the specification of α. Recent theories agree in setting $u \equiv p$, $v \equiv q$, and $\alpha = \frac{2\pi i}{h}$.

[2] R. B. Lindsay, Proc. Nat. Acad. Sci. 17,420 (1931) has thus considered the equation of propagation $\nabla^2\varphi = \ddot{\varphi}/c^2$ for a small disturbance from equilibrium in a compressible fluid medium, which can be split up into two: (1) An equation of continuity $\nabla^2\varphi = -\dot{s}$ and (2) an equation of motion $\dot{\varphi} = c^2 s$. [φ is the velocity potential, C is the velocity of propagation, and S is a quantity, called "condensation," introduced in order to make possible the separation into two equations]. The second equation may be said to deal with the "particle" aspect, since one develops it for a "part of the medium considered as a whole."

established equations into components, one of which will deal with continuity specifications and one independent of it. Both characters are present in equations that work. The "complementarity" is most convincingly demonstrated mathematically. Nevertheless much remains unresolved simply because the mathematicians themselves have had such ill-luck with the problem of the continuum. What physical sense can we derive from a statement like, "A discontinuum includes only discontinua, but the continuum includes the discontinuum within itself" (G. N. Lewis) when mathematicians cannot attach rigorous meaning to the expression, since they are hopelessly split on the meaning of "continuum?" The physicist will have to order various brands of continua (not *the* continuum) which he can use.

Yet, whether we deal with algebras or geometries, discontinua or continua, back of all is the postulational character of mathematics. If physical data determine the choice of a symbolic algebra, it should be remembered that this in turn determines the choice of a set of postulates. We have not seen this point adequately stressed. A guess as to the most plausible reason for this neglect is that it is caused by an actual fear of the problem of putting physics on a postulational basis. The same assertions are presented by different individuals as laws, postulates, principles, or even as experimental facts. It seems to be a very difficult task to transform an abstract mathematical postulate into a "meaningful" physical analogue. We believe that much more will be accomplished if physicists formulate such an approach as a definite ideal, a consciously expressed one. Physical meaning has been sought as a rule in incidental products of mathematical development, whereas it should be sought more assiduously at the fountain-head, in the very postulates, no matter how abstract and forbidding they may seem.

EXPERIMENTAL PROGRESS

The crop of experimental issues, is it as rich as one may expect, and can it be made richer?

The value of the quantum theoretical approach has been well

established by its fruits, a series of clues of things devoutly to be searched for followed by their successful finding. A multitude of spectral studies have been inspired by theory. The notable Raman effect was predicted in a calculation by Smekal several years before its discovery. The story of the varieties of hydrogen (para and ortho) has often been told. The setting up of sign-posts to the isotopes and heavy water; electron diffraction phenomena; the discovery of the positron, the neutron, the annihilation and creation of matter;—all may be cited as instances of theory-provoked or accelerated discoveries. Besides there are discoveries-to-be-made on the waiting line: the neutrino and the negative proton.

Theoretical successes, however, have a way of swinging effort into old but previously neglected directions. The most hopeful of quantum enterprises at the moment is a determined attempt to seize the field of chemistry for physics. The resonance phenomenon, unknown to classical physics, serves as a basis for a valence theory of the constitution and stability of chemical compounds. In principle at least, chemists are hard put to it to find a unique character for their science. The attack is likely to propagate to the frontiers of biology, and closer discussion, on the basis of workable theories rather than arbitrary definitions, may in due time become possible as to whether physics is essentially the one and only natural science or whether there are genuine "emergent" levels among the natural sciences.

The other focus of attention is the vast problem of the nuclear structure of the atoms, involving the fascinations of radioactivity, spontaneous and induced, and the now numerous transmutation reactions and their startling possibilities of release of immense energies.

Two protests may be entered here;—one on the part of experimentalists who have been fooled by erroneous calculations or hate to have their empirical thunder stolen by theoretical lightning (mathematical guesses, to be sure!); another, on the part of those who recognize that there is no pure quantum contribution without admixture of the classical. In deference to such protests, what should one do when figures lie? The decision requires a

Solomon. The customary wisdom is that *no* theory is complete for all contexts of physics, and that *any* theory may be faced suddenly with anomalies.

Finally, there may be here and there die-hard critics, who might maintain, in the face of all novelty, that a one-sided theory leads to a one-sided selection of experimental material, and that one may be missing God knows what by not finishing old issues. It is plain, however, that the burden of proof is on the conservatives in this case.

PHILOSOPHIC PROGRESS

It is of great interest to philosophers whether the developments of quantum theory have brought really new philosophic issues, or have merely thrown new light on old issues. Several scientists of note see a completely new front in every discovery, most trained philosophers do not see any at all. Granted that there is nothing new under the sun of philosophy, there are at least new shadings. And for those science surely sets the fashion. Genuine novelty, too, may be claimed, where science acts in a negative way in uncovering new paradoxes and new dilemmas. They are the severest demands for novelty in philosophy.

Fashion, then, if nothing more significant, has swung under the influence of quantum theory, to a rehash of various extreme idealistic philosophies (mostly in amateur hands), an intensified discussion of topics like causality and continuity, and to a much more serious re-examination of the business of the physicist as he measures and theorizes. The chief tensions provocative of discussion have been: the successes of relativity theory and the simultaneous inadequacy of that theory in the atomic realm; the paradoxes of elementary particles and waves and the resulting critiques of measurements; and the positivistic programs for scientific construction and the opposing influences outside of science for "freedom of the will."

A forced farewell to the naive conception of the objectivity of science has led to a mad scamper to equally naive subjective philosophies and even to a noisy solipsism, which believes in propaganda. A sincere methodological approach to metaphysical

neutrality in the framing of theories had almost been wiped out by a panicky urge to define the ontological status of the theoretical entities of science because of their double rôles, which were declared to be utterly contradictory and paradoxical in spite of the fact the rôles never quite coincided. The metaphysicians of objective and subjective bias seem to have been equally futile. The problem—are the electrons real?—should be dissolved rather than solved. When both opposing *answers* are mad, it were wise to suspect the *question* itself of madness. (The logical positivist formula: objective = intersubjective would possibly constitute a solution if the unfortunate prefix "inter" could be freed of black magic.) It is idle, therefore, to hold that quantum theory has in any essential way changed the issue between science and metaphysics, or the wisdom of a neutrality which should be mutual.

The fuss over causality has brought out what should have been known by all scientists since Hume, that *efficient causality* had long been dead anyhow, but that it had no distinct successor. The attempted enthronement of what we might call *efficient indeterminism* has turned out to be as hopeless as the resuscitation of the dead. "Probability versus causality" was a phantom assault in which the lion and the lamb decided suddenly to lie down together. One is tempted to risk the assertion that for every flaw of "causality" there may be found a corresponding one for "probability," if both are to be taken in the usual narrow sense. This extends even to the higher follies, e.g., the suggestion taking the name of relativity in vain, that if nebulae are accelerating they may (on reaching the velocity of light!) lose "causal connection" with the universe, has its analogues in, let us say, the "disappearance of time" in quantum phenomena. The follies and the partisan debates are ruled out by the *physical* fact that no nebula can quite attain the velocity of light, and that no state can be quite closed. Wanted, a synthesis!

Even though no physicist can rend asunder what the facts unite, it is still fruitful to inquire how indeterminacy arises. The answers place its incidence either in "Nature" or in "the taken," that is, either something objective or something con-

ventional. Objective indeterminacy is, however, a difficult concept for psychological assimilation and rests on the identification of a *somewhat* with a mathematical function. It is that somewhat that is indeterminate and external at the same time. Similarly if the specifications of "the taken" are not completely described there arises a similar perplexity, which is more readily blameable on some limitation within ourselves and is hence credited to subjectivity. Yet both of the answers may be psychological traps for the unwary. Is not there a third way? Possibly, for instance, it were saner to locate the indefiniteness in language itself. We should propose along such lines of thought that the principle might be renamed for clarity as the *principle of ambiguity*,—the ambiguity being the haunting ambiguity of description in general. If, then, we follow Bohr in considering description as a compound of observation and definition, one might derive therefrom sets of ambiguities of observation (measurement) and of linguistic (mathematical) definition. These principles of ambiguity are actually fruitful in formal development. A strange result of the kind to be credited to quantum theory is that by the use of the indeterminacy principle it is possible to pass to a demonstration of Mach's ideal of the elimination of space as an *active cause* even more naturally than by a classical approach. Old issues are thus sharpened in an unexpected way, and a synthesis with relativity is hinted, insofar as a common prescription or postulate is shown to be possible.

The statistical heart of quantum theory focuses attention not merely on the indeterminate in the sense that it is made to relate to the theoretical incompleteness of our knowledge of initial conditions but also to the reappearance of the *a priori* in the foundations of the probability theory itself. Laplace's ghost walks the earth again. One is compelled to start with a priori probabilities somewhere, and one does so when one invokes the "equally likely" without experiment. Other attacks lead to questions like—What is the probability of a probability, and so on? This is either viciously circular or can be resolved only by a much more profound analysis of "possibility" than we have made heretofore. What *do* we mean when we speak of "all possible

arrangements," and what are we to do when the solution is infinity? Nor does experiment completely resolve the difficulty because strictly the independence of probabilities is very likely a fiction, and the dependence of probabilities might, for all we know, obey any law. Hence we either embrace a fiction—independence of probabilities—a necessary one, let us say, or we are faced by a new gigantic problem, namely how many types of statistics in which probabilities are not independent can we set up. The limit to the answers to this problem is the ingenuity of mathematics.

The discussion of the more specific issue of continuity versus discontinuity was touched upon above with respect to its mathemetical aspect, and although the issue was asserted not to reside clearly in the physical situation, it must connect with the physical at some level. What, when and where is an individual is distinct for "macro" situations (above the threshold) and definitely carries along with it a workable theory of measurement and of action and reaction, in which clear-cut notions can be set up regarding scales, clocks, pointer readings and the observer. The same question and the details fade into ambiguities *below* the threshold, the ambiguities of "as if," which cannot, we think, be called properly *physical* at all, but perhaps better linguistic (hypothetical, mathematical). Many, all too many, chess games can be back of the resulting winning moves which alone are apparent at the physical level. In the realm of "as if" there is this greater, fictitious freedom.

Paradoxes of this type form a group. The contrasts between relativity and quantum theory, a macro-physics and micro-physics, will be found in many situations. A very interesting one that has appeared of late (Gamow's work on nuclear physics, etc.) puzzles over the fact that if we accept the elementary particle character of the proton (p) and the neutron (n), as well as the negative electron ($\bar{e}$) and the positron ($\overset{+}{e}$), we can write reactions based on experiments of transmutation:

(1) $$p + \bar{e} \rightarrow n$$

(2) $$n + \overset{+}{e} \rightarrow p$$

On combination these give $\bar{e} + \overset{+}{e} \rightarrow 0$ or annihilation. The converse we might call creation. What in the world, however, can we mean by combination, but a confusion of the *complex* and the *simple*, a paradox of the usual kind? If, now, we imagine reactions (1) and (2) to follow on one another before appearance of a result above the threshold or at the true physical level, the sting is taken out of the paradox. The paradox lies in treating equations (1) and (2) as ordinary simultaneous equations and solving blithely according to rule.

To the writer there seems to be some light in these considerations for a revision of epistemology and even a possible science of *meta-physics* (with apologies for the strain in the term). The persistence, or necessity, of error could be the keynote of a set of principles of such a science. There are many ambiguities of a fundamental kind to be uncovered. The paradox-pairs would come flocking to the ark of such a science; then all situations where infinite regresses can be set up; then significant (linguistic) propositions underlying mathematics; finally a plan of all possible logics and the outline of all possible dialectic procedures to hold the pairs from divorce. Under the banner of "Both-ness." At the same time there would be an abolition of metaphysical terms (old-style), as "reality," "transcendence," etc. Under the sanction of a *science of limitations and synthetic requirements*, even more than under the sanction of mathematics only, physics will perhaps get a new grip on the problem of motion (flux, change, etc.) so that old terms will accommodate changes in meaning. Thus the term "mechanical" need not be limited to inglorious clanking, but may actually include what we might call "emergent motions," mathematical models of limitless variety, and even periodic pulses of alternations of character, e.g. matter and energy both. Only then will the extrapolations of quantum theory to "life" have possible cogency, since the new terms will not permit an unbalance or divorce of the "natural pairs." The cardinal sin of recent extrapolations is that only one factor, the indeterministic, is expanded, and thus distorted. We get contrasts which are utterly artificial. Here is a selection of proposed equivalents one gets by paging recent articles:

Free, as in life	*Physically determined*
individual	continuous
single	secondary
localized extreme oscillation	non-statistical
quantum-like	accurately predictable
Brownian	Newtonian
primitive	subject to law
having inner determination	having outer determination
having determination other than physical	obeying differential equations
initiatory	obeying 2nd Law of thermodynamics
externally uncontrolled	mechanical
something still more ultimate	

The October revolution of physics has still to come.

SUMMARY

Mathematical Progress.

(1) Quantum theory waits for more and better mathematics which will lead to placing physics on a genuine postulational basis.

Experimental Progress.

(2) Quantum theory provokes discovery with little risk of loss of old bearings.

Philosophic Progress.

(3) New light has been cast on philosophic issues, suggesting
- (a) the futility of certain old philosophic muddles, and yet
- (b) the value of cultivating paradoxes from the data of experiment;
- (c) a possible science of "meta-physics" and thus
- (d) a basis for a truly unified science under the banner of physics.

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A Critique of the Species Concept in Biology

BY

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". . . though we cannot strictly define species, they yet have properties which varieties have not, and . . the distinction is not merely a matter of degree."—W. BATESON.

THE PROBLEM

THE species concept is one of the oldest and most fundamental in biology. And yet it is almost universally conceded that no satisfactory definition of what constitutes a species has ever been proposed. The present article is devoted to an attempt to review the status of the problem from a methodological point of view. Since the species is one of the many taxonomic categories, the question of the nature of these categories in general needs to be entered into.

The only biological category possessing an undisputable ontological significance is that of a living individual. Albeit in certain cases, as in those of the symbiotic and colonial forms, the delimitation of an individual meets with difficulties, the category of individual is saved by the admission of the existence of individualities of different orders. However, this category alone is plainly insufficient. We are confronted with (a) a stupendous number of existing individuals, and with (b) their immense diversity. Hence, a coherent knowledge of the living world is possible only with the aid of a hierarchic classification. Re-

gardless of what principle is chosen as a foundation of such a classification, introduction of categories of different orders is indispensable. The category of individual is the lowest in the classification now accepted, that of the species is one of the higher ones. As such, the category of species has a clear pragmatic value.

It remains to be ascertained whether the species is a purely artificial device employed for making the bewildering diversity of living beings intelligible, or corresponds to something tangible in the outside world. Having evolved a classification based on arbitrary categories the investigator may be only too prone to mistake the product of his own mentality for some preëxisting "order of nature." Is, then, the species a part of the "order of nature," or a part of the order-loving mind?

NATURAL AND ARTIFICIAL CLASSIFICATION

It is customary to make a distinction between natural and artificial classifications. A critical evaluation shows that the "naturalness" of a classification may vary quantitatively; more and less natural classifications are possible. A natural classification may be defined as one reflecting empirically existing discontinuities in the materials to be classified. Any apparent discontinuity may serve as a basis for construction of a natural classification. Organisms vary in color, size, in external and internal structures, in their physiologies, in descent, etc. A classification is the more natural the larger is the number of discontinuities it subsumes in each division. An ideal classification would include all the discontinuities, and the knowledge of the position of an organism in such a classification would permit the formation of a sufficient number of deductive propositions for a complete description of this organism.[1]

[1] Since the post-Darwin period a "natural classification" has meant in biology a classification based on the hypothetical common descent of the organisms. This restriction of the meaning of the term is unjustified. The actual mode of descent has been ascertained, and can be ascertained, only for very few groups. But even granting the possibility of establishing the complete phylogenetic history of every organism, it has never been adequately proven that the degree of similarity between the organisms is always proportional to the closeness of their blood relationships. Some palaeontological data cast a grave doubt on this point.

According to the above definition, in a continuously varying living world only a purely artificial classification would be possible. In such a world all the individuals would be arrangeable into a single linear or dichotomic series, in which the extremes would be connected by an infinite number of transition forms (other forms of continuous variability can also be conceived). Applied to a continuous series of forms, a species concept would by no means be meaningless (the opposite opinion has been repeatedly expressed in literature). In fact, a hierarchic classification would still remain the only instrument with the aid of which the diversity of forms might be made describable and intelligible. A perfectly continuous series of forms may be cut at such points, and into as many sections, as deemed desirable for the purposes of the investigator. The segregation of a group of forms into two, three, or more "species" would be determined merely by expediency.

Cases of continuous variability are actually encountered. The species of the fossils are readily separable only as long as the known representatives of a given group from the older strata are scarce. New findings fill up the gaps between the species, until a continuous series of forms finally emerges. A similar condition is observed among some of the now living organisms, principally in the asexually or parthenogenetically reproducing groups. The Bacterium coli—Bacterium typhi complex consists of a large number of strains that differ from each other in their behavior in cultures, in their metabolism, and in pathogenic properties. Some strains differ widely from each other and are readily distinguishable, but the numerous intermediates make the splitting of the group into two, three, or more species equally arbitrary.

And yet, bacteriologists find it more useful to speak in this case about a group of species rather than about a single greatly variable one. Some genera of plants (Rubus, Hieracium, Rosa) approximate this condition rather closely, with a result that the "species" in these genera becomes an arbitrary unit.

DISCONTINUOUS VARIABILITY

Continuous variability is, however, an exception rather than the rule among the now existing living things. A superficial as

well as a most searching investigation reveals not continuums but discrete groups of forms, every member of each group being more similar to every other member of the same group than to any member of any other group. The degree of discontinuity is, of course, highly variable. The intermediates connecting the different groups may be either wholly wanting, or they may be merely less frequent that the modal members of each group. Expressed quantitatively, the organisms do not form a single probability distribution, but rather a large number of separate probability distributions, or of more or less sharply multimodal curves.

The manifest tendency of life toward formation of discrete arrays is not deducible from any a priori considerations. It is simply a fact to be reckoned with. Formation of discrete groups is observed among animals as well as among plants, among the lowest as well as the most complex organisms. It is a universal rule, and the existence of some cases of continuous variability is far from sufficient to invalidate it. A causal analysis of this phenomenon is one of the major tasks of the biological sciences. Strange as it may seem, the predominance of the evolutionary trend of thought in some branches of biology during the last half century diverted the attention of the investigators away from this task rather than toward it, for an evolutionist is frequently more interested in the "bridging the gaps" between groups of organisms than in the nature of the gaps themselves. Only with the advent of genetics has the situation changed somewhat (cf. W. Bateson, "Problems of Genetics").

Discontinuous variability constitutes a foundation of the biological classification. The segregation of a given array of forms into two or more separate groups (be they classes, genera, or species) implies that a corresponding number of discontinuities have been observed among the constituents of this array. As long as such a correspondence is retained, the classification not only serves the purpose of a well-ordered catalogue, but is firmly rooted in phenomenology. The very existence of the classification becomes a symbol of the predominance of discontinuous variability among organisms.

The above reasoning is evidently applicable to the categories

above the species (genera, families, orders), below the species (subspecies, aberrations), and to the species itself. It should be emphasized, however, that the usage of the different categories remains arbitrary, in the sense that a given discrete group of forms may be classed as a genus, subfamily, or a family as may be deemed desirable by the investigator. Only in the case of species this freedom of usage may be restricted by the introduction of a separate criterion, thus making the category of species methodologically more valuable than the rest.

SPECIES AS "SYNGAMEONS"

The rôle of the sexual method of reproduction as an obstacle in the way of the formation of discontinuous groups of individuals has been clearly recognized for a long time. Darwin devoted no little attention to this point, and all the more recent evolutionists were fully aware of it. The development of genetics brought a clarification of the understanding of the mechanism involved. Every discrete group of individuals represents a definite constellation of genes. If the different groups interbreed freely with each other, a new equilibrium is established in which the different genic constellations become fused into a single one. It necessarily follows that no discontinuous variation can exist in a perfectly panmictic population.[2] Mutatis mutandis, the existence of two or more discrete groups of individuals is a proof that free interbreeding between them is prevented by some factor or factors.

It is a matter of observation that closely similar individuals habitually interbreed, and those less similar do not. Hence, a stage must exist in the process of evolutionary divergence, at which an originally panmictic population becomes split into two or more populations that interbreed with each other no longer. A fundamental importance of this stage has been especially emphasized by Bateson, and more recently by Lotsy. Lotsy

[2] This statement applies, of course, only to the discontinuities caused by gene complexes (related species, not to speak about higher categories, differ from each other usually in many genes). Changes in single genes may produce variants so far removed from the original form that their characteristics do not overlap. Discontinuities of this nature may be preserved indefinitely in a panmictic population.

coined the word "syngameon" to designate "an habitually interbreeding community" of individuals. According to Lotsy, a species is a syngameon.

Here, then, we have a definition of species which is especially attractive because of its simplicity. Unfortunately, in its original form this definition meets with difficulties, since by no means all the groups of individuals that are potentially capable of interbreeding actually do so. Colonies of individuals living on islands or in isolated mountain valleys may not interbreed for centuries, and nevertheless their gene structures may be so similar that it were a paradox to classify them as separate species. Similarly, in most of the widely distributed species the communities of individuals inhabiting the remote parts of the specific area never interbreed, though they may be connected by a chain of communities interbreeding to a certain extent with their neighbors.

In spite of the above objections, Lotsy's attempt to clarify the species concept is sound in principle. The emphasis should be placed however not on the absence of actual interbreeding between the different form complexes, but rather on the presence of physiological mechanisms making interbreeding difficult or impossible.

ISOLATING MECHANISMS

Although the mechanisms preventing free and unlimited interbreeding of related forms are as yet little understood, it is already clear enough that a large number of very different mechanisms of this kind are functioning in nature. This diversity of the isolating mechanisms is in itself remarkable and difficult to explain. It is unclear how such mechanisms can be created at all by natural selection, that is what use the organism derives directly from their development. We are almost forced to conjecture that the isolating mechanisms are merely by-products of some other differences between the organisms in question, these latter differences having some adaptive value and consequently being subject to natural selection. The experimental evidence in favor of this conjecture is however scanty in the extreme.

The physiological mechanisms producing isolation may be

divided for descriptive purposes into two classes, namely mechanisms preventing the appearance of hybrid offspring (incompatibility of the parental forms), and mechanisms making the hybrids sterile and, consequently, incapable of propagating further (hybrid sterility). Some examples of each kind are given below.

Sexual isolation (in animals often termed psychological isolation) is very commonly encountered. Males of one species fail to mate with females of the other, or females avoid the alien males. In plants the pollination of the flowers of one species with the pollen of another may be prevented by the structure of the flowers themselves, or a failure of the foreign pollen to germinate properly on the stigma. Failure of spermatozoa of one species to be attracted by the eggs of another, and death or malformation of the developing embryos, are observed in some animals, although these methods of isolation are encountered mostly between rather remote forms.

Since the times of Ormancey and Dufour it is generally believed that unlike matings, especially among insects, may be prevented by dissimilarities in the structure of the genitalia that make copulation impossible. This is termed mechanical isolation. There is no doubt that the mechanical isolation actually exists in some cases (cf. Standfuss's and Federley's experiments on hybridisation in different moths), but in general its importance has been greatly exaggerated. In most insects female and male genitalia are not at all related to each other as a positive and a negative (lock and key according to Dufour), and experimental evidence shows that normal copula is frequently possible between forms having rather differently built genitalia.

Under laboratory conditions some closely related forms cross readily and produce fertile offspring, and nevertheless hybrids between them are rare or absent in nature. This may be due to the parental forms occupying different ecological stations (ecological isolation), or having different breeding seasons (temporal isolation), and consequently never meeting each other except in captivity. A very clear case of this type has recently been described by Cuenot in Sepia officinalis, where two rather similar forms live at different depths in the sea, and each have a rather

short breeding season that does not coincide with that of the other. The ecological and temporal isolations are less dependable in their effects than other isolating mechanisms, and probably many if not most of the naturally occuring interspecific hybrids (especially among plants) come from species that are isolated from each other only in this way.

Hybrid sterility proves to be a common name far rather diverse mechanisms preventing the formation of functional germ cells by a hybrid organism. Probably the best understood kind of sterility is that observed in hybrids between species that are members of polyploid series (i.e. one having a chromosome number which is a simple multiple of that in the other). Such hybrids carry one set of chromosomes from one of the parents, and two or more sets from the other. As shown first by Rosenberg in Drosera, the result is the formation at meiosis of a number of bivalents approximately equal to the number of chromosomes in the parent having fewer chromosomes, while the remaining chromosomes are left as univalents. At the meiotic divisions the univalents are distributed irregularly, causing unbalanced chromosome complements in the resulting cells, which are therefore abnormal and non-functional. Since polyploid series of chromosome numbers are encountered in some plants, and very seldom in animals, this type of sterility is on the whole a rather special case.

In hybrids whose parents have similar chromosome numbers, or numbers that are not multiples of each other, the mechanism of sterility is different from the above. I have pointed out in other publications that at least two kinds of sterility are observed, namely chromosomal and genic sterility. Chromosomal sterility is caused by dissimilarities in the gross structure of the chromosomes of the parental species. The gene arrangement in the chromosomes of related species is often different. Hence, the hybrids become what is known as structural heterozygotes carrying inversions, translocations, and perhaps also duplications and deficiencies for chromosome sections. Since chromosome pairing at meiosis is due to a mutual attraction between homologous loci in the chromosomes, and not between chromosomes as

wholes, an accumulation of structural differences between the chromosomes leads to irregularities or to failure of chromosome pairing in the hybrids. This, in turn, causes abnormalities in the functioning of the cell division mechanisms, and a consequent degeneration of the gametes. There exist some evidence that the sterility of many interspecific hybrids in plants, and also in some animals, is due to these causes.

Some species are so similar in the gross structure of their chromosomes that the sterility of the hybrids between them can not be accounted for by mechanical difficulties in chromosome pairing. Good examples of such species are Drosophila melanogaster and Drosophila simulans (Sturtevant), and the A and B "races" of Drosophila pseudoobscura (Dobzhansky). The sterility of their hybrids is apparently due to the action of complementary genetic factors contributed by each of the parental species. Certain combinations of these factors produce disturbances in the processes of the formation of germ cells in the hybrids, making the latter sterile. It may be conjectured that this type of sterility (the genic type) is most commonly encountered in interspecific hybrids, at least among animals. The classical case of interspecific sterility, that of the mule, probably belongs here.

Other mechanisms of hybrid sterility are probably also encountered (sterility due to an intersexul condition in the hybrids observed by Goldschmidt in Lymantria, the plasmatic type suggested by Michaelis's studies on Epilobium). Moreover, in some cases two or more of these mechanisms combine to cause the isolation of even closely related species. Remarkably enough, an accumulation of genic differences between the parental forms does not necessarily produce either hybrid incompatibility or sterility. The genetic factors responsible for the production of these isolating mechanisms appear to constitute rather a class by themselves. Thus, mechanisms preventing a free interbreeding may apparently develop in forms that are rather similar genotypically, and, vice versa, genotypically more different forms may remain potentially interfertile.

THE STATIC AND THE DYNAMIC CONCEPTIONS OF THE SPECIES

Lotsy's definition of what constitutes a species should be modified thus: a species is a group of individuals fully fertile inter se, but barred from interbreeding with other similar groups by its physiological properties (producing either incompatibility of parents, or sterility of the hybrids, or both). Since the organisms propagating sexually can be experimentally shown to be segregated in nature into discrete non-interbreeding groups, this definition rests on a solid factual basis. However, an attempt to apply this definition in practice will soon show that in some cases classification of two groups of individuals as separate species or as varieties of the same species remains arbitrary. The reason for this is exceedingly simple: neither the mechanisms producing incompatibility, nor those producing sterility, function on an all-or-none principle. For instance, sexual isolation may be incomplete, and individuals belonging to different groups may sometimes, though seldom, copulate. Similarly, some hybrids are only semi-sterile, or sterile in one sex only.

An analysis of the situation shows where the difficulty lies. The concepts of the taxonomic categories, as all the taxonomic concepts, are essentially static. Taxonomy attempts to isolate from the actual experience a sort of a photographic image of the living world, and to give a comprehensible description of the regularities observed in this image. The concepts of taxonomy are derived from reality by a process of abstraction, which leaves out of consideration the dynamism which is one of the most essential, if not the most essential attribute of life. By this method the flowing and changing patterns of life are made to conform more or less to the static limits of the taxonomic categories. But actually the taxonomic categories in general, and species in particular, are not static but dynamic units. The discontinuity of the living world is constantly emerging from a continuity, while continuity tends to increase in extent as a result of the extinction of some of the discrete groups previously formed. The unstable equilibrium between these two opposing trends, and the factors tending to displace it in either direction constitute one

of the main problems of any theory of evolution. Considered dynamically, the species represents that stage of evolutionary divergence, at which the once actually or potentially interbreeding array of forms becomes segregated into two or more separate arrays which are physiologically incapable of interbreeding.

The fundamental importance of this stage is due to the fact that it is only the development of the isolating mechanisms that makes possible the coexistence in the same geographic area of different discrete groups of organisms. As pointed out above, every discrete group owes its distinctness from all other groups to a definite and unique system of genes it carries. As long as such groups are virtually fully interbreedable, they can remain separate only if they inhabit different and sufficiently remote geographic regions. The boundary lines between such groups become rapidly obliterated soon after they come in contact in the same locality, and free interbreeding sets in. On the other hand, development of isolating mechanisms renders the differences between groups relatively fixed and irreversible, and permits them to dwell side by side without loosing their differentiating characteristics. This, in turn, opens the possibility for the organisms dwelling together to become adapted to different places in the general economy of nature. The usage of the term "species" can and should be made to reflect the attainment by a group of organisms of this evolutionary stage.

SPECIES FROM THE GENETIC AND THE TAXONOMIC STANDPOINTS

In cross-fertilizing organisms species may be separated from each other on the basis of the presence or absence of physiological hindrances to interbreeding. An accurate determination of the degree of isolation attained by a given pair of presumed species is possible, of course, only by means of an experimental analysis of their relationship. The question immediately presents itself: to what extent do the species thus delimited coincide with the species of taxonomists? Indeed, in a tremendous majority of cases taxonomists have no direct evidence bearing on the crossability of forms which they describe as separate species. Although a detailed discussion of this problem lies outside of the

scope of the present article, it should be pointed out that the two classifications are of necessity not too dissimilar. The criteria used by taxonomists for separating species (especially clearly formulated by A. P. Semenov-Tian-Shansky) give a sort of circumstantial evidence for distinguishing non-interbreeding groups of forms from those that regularly interbreed. Taxonomists assume that different species exhibit different cycles of variability separated from each other by more or less clearly pronounced gaps, while varieties of the same species belong to the same cycle of variability and are not separated by such gaps. Similarly, discrete groups coexisting in the same locality without producing intermediates are considered separate species, while forms replacing each other in different geographic regions (so that a single form occurs in every region), and grading into each other in the intervening regions, are classed as subspecies or varieties. Discrepancies between the "genetic" and the "taxonomic" species are to be expected mainly in those relatively rare cases where different groups do not interbreed despite the scarcity or absence of morphological differences between them (a good example of this sort are the "races" of Trichogramma minutum described by Harland and Atteck), or where geographically isolated races, without losing the ability to interbreed, have diverged so widely in their morphological characters that taxonomists feel compelled to consider them separate species (for instance, some "species" of pheasants).

Among organisms reproducing exclusively by parthenogenesis or asexually, species in our sense do not exist at all. The classification of these organisms must be based solely on the observable discontinuities in their morphological structures and physiologies. Methodologically this classification is similar to the combining of species into higher taxonomic groups.

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Psychology versus Immediate Experience[1]

BY

EDWARD CHACE TOLMAN

IN THIS paper I am going to try to indicate my notion concerning the nature and subject-matter of psychology.[2] I am a behaviorist. I hold that psychology does not seek descriptions and intercommunications concerning immediate experience[3] per se. Such descriptions and attempts at direct intercommunications may be left to the arts and to metaphysics. Psychology seeks, rather, the objectively stateable laws and processes governing behavior. Organisms, human and sub-human, come up against environmental stimulus situations and to these stimulus situations they, after longer or shorter intervals of time, behave. The laws and processes determining this their behavior are stateable in objective terms. Even in the cases where the organism is oneself, these determining causal factors can and must—for the purposes of psychology—be stated objectively. It is true that in these latter instances, in

[1] Presented before the Kosmos Club of the University of California. February, 1934.

[2] For a previous but less completely thought through statement of the position to be presented here see E. C. Tolman, *Purposive Behavior in Animals and Men*, New York, The Century Company, 1932, especially Chapters XXIV and XXV.

[3] I shall throughout use the phrase "immediate experience" to designate the immediately given pre-analytical complex (to borrow Professor Loewenberg's term. Cf. J. Loewenberg, Pre-analytical and post-analytical data, *J. Philos.*, 1927, *24*, 5–14.) as this appears to the naïve man and before the subtilities of philosophical and scientific analysis have been applied to it.

which the animal in question is oneself, one may in one's rôle, not of a psychologist, but of an artist or a metaphysician, attempt to describe and convey to another man one's own facts of immediate experience. But such a description and report of immediate experiences, except in so far as this report is itself a form of behavior and therefore like all other behaviors the basis for an investigation of the objective laws and processes underlying it, essentially new to the picture. Experience *qua* experience, while of concern and interest to the man in the street, the philosopher and the poet, does not enter as such into the laws and equations of psychology,—in so far, at any rate, as psychology is to be considered as a science.

will not, for the purposes of the psychologist, add anything

In order, however, to lend such a doctrine cogency I need to begin at the beginning. I must attempt to review, at least briefly the situation relative to the problem of knowledge as this problem has in the past appeared to the philosopher and to the psychologist, and as it now appears to me.

Ever since the time of Descartes the so-called relativity of sense perception has been of prime concern to the philosopher. It is because of the facts of sense relativity that there arose most of the technical issues of modern philosophy. And it is largely in terms of those facts that the traditional, pre-behavioristic, notion of the subject-matter of psychology arose.

By way of recalling to you what is meant by this matter of sense relativity let me quote some passages from Bertrand Russell.

"To make our difficulties plain," he says, "let us concentrate attention on the table. To the eye it is oblong, brown and shiny, to the touch it is smooth and cool and hard; when I tap it, it gives out a wooden sound. Any one else who sees and feels and hears the table will agree with this description, so that it might seem as if no difficulty would arise; but as soon as we try to be more precise our troubles begin. Although I believe that the table is 'really' of the same color all over, the parts that reflect the light look much brighter than the other parts, and some parts look white because of reflected light. I know that, if I move, the parts that reflect light will be different, so that the apparent dis-

tribution of colours on the table will change. . . . It is evident . . . [therefore] that there is no colour which preeminently appears to be the *the* colour of the table, or even of any one particular part of the table . . . it appears to be of different colours from different points of view, and there is no reason for regarding some of these as more really its colour than others. Thus colour is not something which is inherent in the table, but something depending upon the table and the spectator and the way the light falls on the table."[4]

He then goes on to indicate that the situation is similar for all the other properties of the table. Not only its color but its texture, its shape, its size depend upon, are relative to, the position and character of the percipient. And, if we include also the facts of illusions, dreams, false memories and hallucinations with these of sense-perception we are easily led to the dogma of Bishop Berkeley that there is no "real" table but only "ideas" of a table. Or, if one will not go as far as that, then one may be led at least to the position of the dualists, who, since there do seem to be some quite independent features of order and character in our sense-perceptions, hold that the world is to be dichotomized into two sets of entities. On the one hand, there are the immediate sense-data—the immediate colors, feels, shapes, images, etc. in and of themselves. No misconceptions, errors or illusions about these sense data are possible. They are what they are. They are immediately given. But, on the other hand, there are also according to this doctrine independent material objects behind and causing such sense-data. These independent material objects are known mediately. They are known indirectly through the sense-data.

It was the assumption of this sort of a dualism which became the starting point for modern physics and modern pre-behavioristic psychology. And it must be granted that the sort of a picture therein presented still dominates much of our thinking. We still often tend to conceive of the mental as an array of immediately given sense-data and to think of different sets of

[4] Bertrand Russell, *The Problems of Philosophy*, New York, Henry Holt and Company Home University Library of Modern Knowledge. No. 35, p. 11f.

these arrays as private to each one of us. The physical, on the other hand, we tend to conceive as an independent and for the most part only mediately known set of entities behind these our several private sensory arrays. The fact that such a view persists means, of course, that it is, in some degree and in some manner, pragmatically workable. I shall, nevertheless, in the rest of this paper attempt to get you to adopt a somewhat different view. I shall abandon this conception of two sets of metaphysical stuffs—a physical world really "out there" to be studied by physics and a series of private mental worlds each in our own respective heads or minds to be studied by psychology. Such a conception does not seem to be as successful for a philosophy of science as does the more modern view for which I am now going to argue.

I shall hold that immediate experience just as it appears, contains quite as much objectivity as it does subjectivity. Immediate experience, as initially given, is not my private world or your private world. It is not something to be studied primarily by psychology. It is, rather, an initial, common matrix out of which both physics and psychology are evolved. It is the only tangible real that we have. Physics does not present another real behind that of immediate experience. Nor does psychology, as such, study this real of immediate experience in a more first hand way than does physics. Physics is a set of logical constructs—a set of rules and equations whereby we are aided in finding our way about from one moment of immediate experience to another. Further, and this purports to be the only new and specific contribution of this paper, psychology is, I shall argue, but another such set of logical constructs, another such set of rules and equations, which, when added to those of physics, will give us still further aid in finding our way about from one moment of experience to the next.

There is, if you will, still left in my universe a dichotomy, but it is a dichotomy not between physical entities, and mental entities, but between both of these as mere logical constructs, on the one hand, and immediate experience as the actually given, rich, qualitied, diffuse, matrix from which both sciences are

evolved, on the other. The purpose of science, of psychology as well as of physics, is not to describe and re-live experience but merely to explain it,—to help in predicting and controlling it,—or to use Professor Pepper's term, to give map accounts of it.[5] My dichotomy will be between reality and its maps and not between two types of reality.

By way, now, of introducing this point of view more specifically let me return to the facts of sense perception. And let me present them again in the light of some of the more recent work in this field.[6] In particular, I have in mind the experiments and theoretical analyses of Brunswik.[7] Brunswik's doctrine is that in any perception the organism "intends" certain objective environmental characters. The absolutely correct values of these characters can be determined only by measurement—that is, by the sorts of procedure introduced and exploited by physics. But immediate perception makes a stab at arriving at these characters or, to use Brunswik's phrase, perception "intends" or "expects" these characters. Further, these perceptual intentions and expectations succeed in "attaining" or "achieving" the real values of the characters sometimes with more and sometimes with less success. Again, these environmental characters which Brunswik finds to be "intended" in perception can, for the purposes of the present argument be divided into two sub-classes: (a) characters which are relatively independent and which may be said to inhere in the things or bodies themselves; and (b) characters which are dependent and determined not only by the character of the thing in question but also by the special relations of this thing to the percipient. The first I shall label "independent properties or qualities" or simply "independents" and the second, "perspective

[5] S. C. Pepper, Categories—Studies in the problem of relations, *Univ. Calif. Publ. Philos.*, 1930, *13*, 73–98.

[6] That is, such work as that of Brunswik, Katz, Hering, Bühler, Gelb, Koffka, Kardos, Thouless, E. R. Jaensch, on the so-called problem of "Dingkonstanz." For references, see L. Kardos, Ding und Schatten—"Eine experimentelle Untersuchung über die Grundlagen des Farbensehens." *I. Abst. Zeitschr. f. Psychologie Ergänzungeband 23*, 1934, and E. Brunswik vide infra.

[7] Egon Brunswik, *Wahrnehmung und Gegenstandswelt—Grunglegung einer Psychologie vom Gegenstand her*, Deuticke, Leipzig und Wien, 1934.

properties or qualities" or simply "perspectives." It also appears that in the usual case the individual is much more likely in perception to "intend" an "independent" than he is to "intend" a "perspective." Furthermore, it appears that he is likewise more nearly successful in perceptually "attaining" or "achieving" independents than he is in perceptually attaining or achieving "perspectives." His perceptual expectations are usually more nearly verified by the corresponding measurement when he has intended the independent object than when he has intended the perspective of that independent object.

Let us take a concrete example. Suppose you now actually look, at two tables or two men—one, say, twenty feet away and one, say, ten feet away. Both, I feel sure, will now look to you as about the same size. In other words, the chances are great that, at this moment without any special preparation, you will perceptually "intend" and approximately "attain," not the perspective sizes, but the independent sizes of these two tables or men. These independent sizes are the same irrespective of their relative distances from you. If, on the other hand, you now set yourselves to perceive the "perspective" or "projective" sizes you can probably, to some extent, do so, but your degree of attainment will, I am sure, be far less than when you perceived the independent sizes. That is, I defy any one of you, unless you have had special training, now to "see" the twenty-foot-away table or man as twice as small as the ten-foot-away table or man. Only, if you have had considerable special training as an artist or if you abandon perception per se and resort to a measuring technique such as holding up a pencil and marking off the projections of the two tables upon it, will you succeed in somewhere near approximating these their relative perspective sizes.

And the situation, which thus holds for perception of sizes, also holds in analogous fashion for the perception of all other qualities, such as for example as color. When one perceives the color of yonder table one may intend either the true independent color or the immediate perspective brown or white or yellow which is due to one's specific position and to the special lighting of the moment. But the surprising thing is that also in this case

of color what one more often intends and approximately attains in his seeing is the true independent object-color and not the momentary perspective color. Russell was, in short, factually wrong when he implied that for all of us and ordinarily "the parts that reflect the light 'look' much brighter than the other parts, and that some parts look white because of reflected light." Actually, it is usually only in our moments as would-be artists or as would-be introspective psychologists that we can intend and come near attaining these immediate perspective values. For most of us, most of the time, the table looks the same brown all over.

Or consider the case of shape. One of Brunswik's students did an experiment in which the subjects had to compare the shape of one ellipse rotated out of the face-on plane with the shapes of a series of ellipses of different proportions lying squarely face on. And it was found that the rotated ellipse tended, unless special instructions were given, to be identified with a face-on ellipse having approximately its, the rotated ellipse's, true shape and not with one having the rotated ellipse's fore-shortened shape. Again, the independent quality, in this case, shape, was perceptually intended and approximately attained and not the immediate perspective quality.

In a word, it appears that in all the different modes of sense perception the subject may perceptually intend and more or less successfully achieve or attain either an independent or a perspective. But he more frequently intends the independents than he does the perspectives. And his degree of perceptual achievement or attainment also is usually greater when he intends the former.[8]

If we return, now, to our initial discussion, it appears that

[8] It is, of course, to be noted that in the case in which he perceptually "intends" an independent—the special circumstances of the latter, such as its distance, its special lighting, its plane of rotation and the like also affect the individual's sense-organs and must so to speak be taken into account by the perceptual apparatus, if the approximately true values of the independent is to be attained. But this the perceptual apparatus inevitably does, and the final product in no way presents psychologically a more "derived" character to the percipient than in the case where he is perceptually intending not the independent but the perspective.

what we there called sense-data we are now calling perspectives and that what we there called the reality behind such sense-data we are now calling the independents. But such a distinction no longer seems fundamental. The really important distinction now seems to lie rather between both perspectives and independents as perceptually intended, expected and more or less successfully attained environmental object characters, on the one hand, and the to-be-discovered laws and equations which determine the nature and functioning of such "intentions," "expectations," and degrees of "attainment," on the other.

Immediate experience is a matrix which contains *both* perspectives *and* independents perceptually (and also, as we shall see, memorially and hypothetically) intended. It also contains the rudiments of measurements, that is, it contains the moments in which these perceived, remembered, or hypothesized qualities are checked by measurement and found to have been more or less successfully attained. Immediate experience as thus composed is rich, qualitied but perhaps in large part, ineffable, that is, logically incommunicable from one sentient being to another. It is, however, real—the most real reality that we can have or desire. Its lack is that it does not provide in itself, or only in minor degree, its own rationale. It does not contain written on its face the rules whereby the particular perceptual, memorial, or hypothetical intentions occur nor whereby these prove to have been more or less successful in their attainment. It is such rules which physics and psychology taken together seek to supply.

As to the difference between physics and psychology my further thesis will now be that whereas physics is the system—the rationale—which attempts to explain all the possible "independents" and "perspectives" any organism could conceivably intend and attain in given environmental set-ups, psychology is the system—the rationale—which attempts to explain what perspectives and independents the particular organism or type of organism, on the given occasion, will actually (perceptually, memorially, and hypothetically) intend; and why these intentions will have the specific degrees of success or failure of attainment that they do. In the rest of this paper I shall attempt to

elaborate this thesis for psychology. The "logical positivists," that is such men as Wittgenstein,[9] Schlick,[10] Carnap[11] in Europe and Bridgman,[12] C. I. Lewis,[13] Feigl and Blumberg[14] in this country, have already done the task for physics. But no one as yet, so it seems to me, has satisfactorily done it for psychology.[15]

But first let me point out that, since according to this doctrine psychology will be concerned only with developing a system of *rules or equations*, this absolves it from any attempt at trying to depict what is going on in the other man's mind. As psychologists we do not seek to re-live and describe the other man's immediate experiences. Such a re-living must be left, as we have said, to metaphysics, or to poetry, or to common sense—that is, to whatever disciplines as may concern themselves with immediate experience, per se. Psychology, as such, is concerned only with such objectively definable variables as the intentions, expectations, attainments, immanent in the behavior of organisms. Psychology as such is objective and behavioristic.

But behaviorism, as we shall now see, may take either of two courses. On the one hand, it may seek to develop a set of what may be called molecular or microscopic concepts. These are essentially physiological. This form of behaviorism attempts a complete neural, glandular and visceral picture. And a large part of what today is known as behaviorism is of this form. But there is also, I would contend, a second type of behaviorism which seeks rather to develop a set of molar or macroscopic concepts concerning what I shall designate as "behavior-readi-

[9] L. Wittgenstein, *Tractatus Logico—Philosophicus*, Kegan Paul, 1927.

[10] M. Schlick, Positivismus und Realismus, *Erkenntnis*, 1932, *3*, 1.

[11] R. Carnap, *Der Logische Aufbau der Welt*, Weltkreis-Verlag, 1928.

[12] P. W. Bridgman, *The Logic of Modern Physics*, Macmillan, 1927.

[13] C. I. Lewis, *Mind and the World Order*, Scribner's, 1929.

[14] H. Feigl and A. Blumberg, Logical Positivism, *J. Philos.*, 1931, *28*, 281.

[15] Carnap and Feigl have, to be sure, prepared the way, but they have done it only for "molecular behaviorism" and not for "molar behaviorism." R. Carnap, Psychologie in Physikalischer Sprache. *Erkenntnis*, Vol. 3, 107–142. H. Feigl, Logical analysis of the psychophysical problem. A contribution to the new positivism. *Philosophy of Science*, 1934, *1*, 420–445. A similar point of view to that presented here but not worked out in detail for psychology seems to be that of the zoologist, Russell. Cf. E. O. Russell. The study of behaviour, *Nature*, 1934, *134*, 835–839.

nesses." This second form of behaviorism seeks to describe and explain behavior in terms of such "readiness-variables." And these latter it fashions directly from the more surface facts of behavior itself. This molar form of behaviorism does not deny the possibility and need of neural and glandular concepts but it prefers to leave the latter to the adequately trained physiologist.

Both varieties of behaviorism start with the same initial facts: (a) the fact that organisms, human and sub-human, come up against environmental stimulus-situations and suffer from internal physiological disequilibriums; (b) the fact that as a result of such stimulus situations and such internal upsets these organisms behave; and (c) the fact that the nature of this their resulting behavior is determined by a set of intervening variables to be conceived as lying in the organism. But, whereas the molecular behaviorist—that is, the behaviorist with physiological bent—seeks to state the nature of these intervening variables in terms of such concepts as chemical or other physical action in the sense organs, innate and acquired neural pathways, sympathetic system reverberations, and the like, the molar behaviorist seeks to state the intervening variables as specific types of behavior-readiness or, in more common sense terms, as objectively definable "demands," "intentions," "expectations" and "attainments." The physiological behaviorist states the laws of visual perception in terms of photochemical actions on the retina, convergence of the two eyes, bi-retinal disparity, lens accommodation and neurological processes in optic thalami and cortex. The molar behaviorist states the laws of perception in terms of objectively defined behavioral conditions of "intention" and "expectation" as these have been discovered by Brunswik and his students, and as they may also be discovered by animal psychologists. There will, of course, be no conflict between the two behaviorisms. Each will play into the hands of the other. The molecular physiological variables will, of course, underly and may, if you will, be said to explain the molar variables of "demand," "intention," "expectation." But these latter still have to be discovered and schematized at their own level. In other words, the psychologist in the guise of the molar behaviorist,

still has, I would assert, his own job to do and can still properly demand his own place in the sun.

But let me be more specific. Both types of behaviorism begin by listing four independent or causal variables, as follows: (1) the environmental stimulus set-up, this may be symbolized by S; (2) the specific heredity of the given organism, this may be symbolized by H; (3) the specific past training of the organism, to be symbolized by T; and (4) a releasing internal condition of physiological appetite or aversion, to be symbolized by P. The independent variables S, H, T and P are the initial causes for the final resulting behavior. And, if we symbolize the latter by B, then a total equation of the form

$$B = f_1 (S, H, T, P)$$

can be written.

The determination of the actual form of this function f_1 will constitute the final goals of both molecular and molar behaviorisms. But, further, as has already been implied, both these disciplines are forced to take one more step in common. That is, they both find that in order to proceed, they now have to subdivide this one final equation into two sets of subordinate equations, or the forms

$$B = f_2 (I_a, I_b, \cdots I_n)$$

and

$$I_a = f_3^a (S, H, T, P)$$
$$I_b = f_3^b (S, H, T, P)$$
$$I_c = f_3^c (S, H, T, P)$$
$$\text{etc.}$$

In these equations, I_a, I_b, . . .I_n represent what I have called "intervening variables." And it is obvious that once these have been properly chosen and the functions f_2 and f_3 been determined then it will be possible to solve for the final total equation $B = f_1 (S, H, T, P)$. But at present both molecular and molar behaviorisms are far from this final step. Both are still trying to work out

their respective intervening I's. And only very tentatively and inadequately are they as yet making suggestions concerning f_2 and the f_3's.

But now, finally, let me turn to the task of stating my own particular conceptions concerning the I's and their connecting functions. I define the I's as behavior-readinesses. And I would divide them into two main groups which I shall designate, respectively as *demands* and *cognitions*. In attempting to illustrate for you the nature of these latter I shall for the most part present the case for rats. For these are the organisms that I know most about. I will, however, as a sop to you as my readers throw out a few suggestions now and then relative to human beings. Turn, first, to the demands. These are to be further differentiated into (1) *ultimate demands* and (2) *subordinate demands*.

(1) By the *ultimate demands* I would conceive behavior-readinesses which appear in, and are defined by, three sets of facts: (a) the going off of positive and negative consummatory responses; (b) the propensity to persist through trial and error to and from locales where the goal-objects appropriate to such positive or negative consummatory responses have been found on previous occasions; and (c) the tendency which appears for an animal in successive trials to select shorter routes to and from such locales. These three behavioral facts I take as indicative of, or as the logical positivists might say, as the pointer-readings for, three types of ultimate demand which I shall designate as: (i) the demand *for* a positive goal-object; (ii) the demand *against* a negative goal-object; and (iii) the demand *for* "short" distances to a positive or from a negative goal.

(i) Under the heading *demands for positive goal-objects* I conceive a set of variables (i.e., behavior-readinesses) such that when any one of them is operative it will act to produce a specific positive consummatory response, if the appropriate goal-object be present. It will also operate, if this goal-object be absent, to produce a persistent exploratory activity until an instance of this appropriate type of goal-object has been found. The list of such positive demands would, in the case of rats, be comprised, I

believe, by (a) the demand for food; (b) the demand for sex-objects; (c) the demand for explorable alleys; (d) the demand for dark places in which to hide; and (e) the demand for warm soft places in which to go to sleep.

Turning to human beings, I suspect that the list of the ultimate positive demands would be much the same. It also is probably largely, if not wholly, comprised by the demands for food, for sex-objects, for explorable alleys, for dark places in which to hide, and for warm soft places in which to go to sleep. It may, however, turn out that in addition there must also be included in the list for men an ultimate demand for harmonious sense-impressions, an ultimate demand for the company of one's fellows and perhaps an ultimate demand for the social approval of one's fellows.

(ii) *Demands against negative goal-objects.* Under this head of negative demands I would conceive a set of variables which would be respectively operative in producing (in the case of rats) negative consummatory responses (a) in the presence of electric grills, (b) in the presence of surrounding and hampering barriers (including the ends of blind-alleys), (c) in the presence of open exposed fields, and (d) in the presence of powerful noxious stimuli. Each such negative demand would also operate to produce a persistence through trial and error to get away from a locale where an instance of the given type of negative goal-object has already been found.

For men also I suspect the list of ultimate negative demands would be much the same. For them also it would, I believe, include and probably be wholly comprised by: demands against electric grills (or other pain-inducing devices), against surrounding and hampering barriers, against open exposed fields, and against powerful noxious stimuli.

(iii) By *demands for "short" routes* I conceive still another set of intervening variables. That is, "demands for short routes" would be my name for the tendencies which appear in behavior to select, after learning, short paths rather than long ones to positive goal-locales and from negative goal-locales. Furthermore these tendencies to learn to take the short routes include

not only the tendency for spatially short routes but also that for temporally short ones and finally that, also, for what, for want of a better name, we may call force-resisting short routes. For it appears that, if there be two alleys to the same positive goal or from the same negative goal which differ in spatial lengths, rats will learn (within the limits of their ability to discriminate) to take the spatially shorter.[16] Similarly, if there be two paths of equal spatial lengths but of which one involves a longer temporal distance than the other—due to the insertion of a longer delay chamber—rats will also learn (again within the limits of their abilities to discriminate) to select the temporally shorter.[17] And, finally, if there be two strings, with food cans attached, which are of equal lengths and which take approximately equal times to pull in but of which one has a lighter weight attached to it than the other, again rats will learn to pull in first (within the limits of their abilities to discriminate) that with the lesser force-resistance—that is, that with the lighter weight attached.[18]

And for human beings, also, I suspect that this same list of demands or tendencies for short routes likewise holds. For men likewise, seem wherever they can, to take the spatially short, the temporally short, and that which in terms of the force to be overcome is "short."

(2) But let us turn now to my second main group of demands—the *subordinate demands*. Under this heading I shall lump (a) demands for specific types and instances of goal-objects and (b) demands for specific means-objects. In the case of rats there is some slight evidence for the (a)-type in that it appears that these animals perform less well, when a new food is substituted at the end of the maze in the middle of learning for one used up until that time. This evidence, however, is not very overpowering.[19]

[16] Cf. J. G. Yoshioka, Weber's law in the discrimination of maze distance by the white rat, *Univ. Calif. Publ. Psychol.*, 1929, *4*, 155–184.

[17] Cf. E. G. Tolman and C. F. Sams, Time discrimination in white rats, *J. Comp. Psychol.*, 1925, *5*, 255–263.

[18] T. L. McCullough, Performance preferentials of the white rat in force-resisting and spatial dimensions, *J. Comp. Psychol.*, 1934, *18*, 85–112.

[19] M. H. Elliott, The effect of change of reward on the maze performance of rats, *Univ. Calif. Publ. Psychol.*, 1928, *4*, 19–30.

And for the (b)-type of case—that in which a characteristic and specific type of means-object sets up in its own right—there is in rats still less evidence. In other words, these animals have not as yet been shown to become permanently attached to, say, pink alleys rather than to white or, if you will, to California redwood mazes, rather than to the white pine mazes imported from New England.

For men, on the other hand, both the above kinds of subordinate demand seem to make up the very breath and substance of life. In short, the whole body of both what the anthropologists find in the way of specific culture-patterns and what psychologists find in the way of individual idiosyncrasies seem to consist for the most part, psychologically speaking, in acquired specifications of ultimate goals or in acquired adherences to specific types of means-objects, which latter then often set up in their own right. And such specifications and settings-up, once established, acquire a strangle hold. If a man grows up in a social group in which one type of food, sex-object, explorable alley, dark place in which to hide, or warm soft place in which to sleep, is current, and then is transported into another social group in which other types of food, sex-object, dark place, and feather bolsters instead of blankets are provided, he will most certainly languish and he may even die. And a man does die, or almost so, for Camels rather than Lucky Strikes, for Bull Dog Drummond rather than Prince Albert, for food at the Men's Faculty Club rather than the very same food—only better—at the Women's Faculty Club. And we professors would come to love our money, if we had it, not only for what it would buy, but for its own sake. And, as it is, we have some of us become misers as to our books and even some few of us as to our pocket matches.

The above is a very sketchy account. But it indicates the general nature of my doctrine that ultimate and subordinate demands are one type of intervening variable—one type of "behavior-readiness" or "I"—between stimuli, physiological conditions, heredity and training, on the one hand, and final behavior, on the other. We have, however, not only the task of discovering these I's, but also that of discovering the forms of the

functions f_3 which attach them to the initiating S's, H's, T's and P's as well as the form of the function f_2 which connects them, along with the cognitions, to the final resulting behavior, B. But these are questions all still for the future. One can, perhaps, already guess that it is probably a very simple function which attaches the ultimate demands to the physiological conditions, the appetites and the aversions of the given species—(i.e., to P and to H). But the further complications which arise in the cases of subordinate demands are still very far beyond us.

Let us turn now to the cognition variables. They also I shall divide into two subtypes which I shall call (1) *differentiations* (*i.e., intentions, expectations, and attainments as to qualities*) and (2) *hypotheses* (*i.e., intentions, expectations, and attainments as to relations*).

(1) It is under the *differentiations* of quality that the perceptual intentions and attainments investigated by Brunswik and his students would be classed. And it is pertinent to our present argument now to emphasize that the procedures and resulting definitions of these workers were perfectly objective. A subject was required, say, to pick out of a series of cubes of varying sizes one, which at a distance of 6 meters appeared equal in size to a standard 6 cm. cube at a distance of 1 meter. If under such conditions he picked out, on the average, a cube about 7 cm. on a side, it could be concluded that he was intending the independent size of the 6 cm. standard cube and missing it by about 1 cm. If, on the other hand, he picked out a cube at 6 meters about 15 cm. on a side, then it could be concluded that he was intending the perspective size of the standard 6 cm. cube. But in this latter case his attainment was proportionally far less. He should have picked out not a 15 cm. cube but a 36 cm. cube at 6 meters to equal the 6 cm. cube at 1 meter.

The experiments were, in essence, purely behavioristic. They were the kind of experiment we can and do do with animals. The only difference is that with animals we usually require them not to equate but to discriminate between a given pair of entities. We teach an animal to go to the larger of two doors or, in the case of a chimpanzee, to pull in with a stick the larger of two boxes.

We teach him this first when both doors or both boxes are at an equal distance from him. But then we place the actually larger box or the actually larger door at a greater distance, so that it now subtends a smaller visual angle—presents a smaller perspective size.[20] And we discover perhaps to our amazement that even under these latter conditions he will still choose this same objectively larger box or door, in spite of the fact that it now "looks" smaller. That is, we discover that the animal, as well as the man under ordinary conditions, perceptually "sees," that is, intends and approximately attains, the independent sizes of objects and not their momentary perspective sizes.

Turn, now, to memorial differentiations of qualities. These likewise are to be discovered and defined quite objectively. Consider, for example, the so-called "delayed reaction" experiment as used with the lower animals. The animal in question is taught first that of two doors, the one which in any given trial is lighted, always leads to food, whereas the other always leads to punishment or to lack of food. He is then introduced into the choice chamber while the light is still on, but the latter is turned off for a longer or shorger time before he is allowed to make his choice. If after this delay-interval he can still choose correctly, we are able to say, quite objectively, that there was operative in him at the moment of choice an intervening variable to be called the memorial differentiation of the previously lighted character of the given door. A certain past but now absent environmental quality is determining his present behavior.

And when one examines the so-called memories not of the lower animals but of one's fellow men one's procedure is really the same. One discovers from their words, if not from their gross behaviors, what the past environmental properties are to which they are now reacting. And one can check up on those past

[20] W. Köhler, Optischen Untersuchungen am Schimpanzen und am Haushuhn, *Abh. d. Kgl. Preuss. Akad. d. Wiss.*, 1915, Phys-math. Kl. Nr. 3, p. 29.

———, Ueber eine neue Methode zur psychologische Untersuchung von Menschenaffen, *Psychol. Forsch.*, 1922, *1*, 390–397.

For similar experiments indicating that hens respond to independent colors and not perspective colors see D. Katz und G. Révész, Experimentelle Studien zur vergleichenden Psychologie, *Zt. f. Angew. Psychol.*, 1921, *18*, 307–320.

environmental properties so as to measure the degree of attainment in these their present memorial assertions. Their words will not provide any essentially different type of information from that which one obtains from the non-verbal behavior of a rat or of a chimpanzee. With all three species one presents certain objective stimulus situations and observes certain resulting successes or failures in a later behavior. And to explain such successes in all three cases one is forced "logically to construct" a type of intervening variable to be called the memorial differentiation of a past environmental quality or property.

It is, nonetheless, interesting and seemingly significant that in the case of men we do thus have, in addition to gross behavior with its successes or failures, verbal reports. And it will be of course part of our final task as psychologists to decide upon the further nature and meaning of such verbal reports. At the moment, however, I shall make but one suggestion with regard to them. I shall suggest that an essential feature of a certain type of verbal report or introspection is that, in it, the individual "intends" his perspectives as well as some independent. A perspective is, as we have seen, perfectly objective. The size of the projected image resulting from a given cube, placed at a given distance, is just as objective as is the independent size of that cube. But I believe that it will be found that it is only in introspection that an organism can actually intend and behave relative to such projected images. In fact, no experiment so far as I know has as yet ever appeared in which even a chimpanzee, to say nothing of a rat, could be shown to be responding, not to the independent size or shape or color of an environmental object, but merely to its momentary perspective size or shape or color. This is not to say that the values of the perspective sizes, shapes and colors do not affect and indeed often distort an animal's response to the corresponding independents. It is, however, to say that an animal's behavior always seems to "intend" the independent qualities and never the perspective ones.

And so it is also for memory. Rats or chimpanzees can never seem to intend merely the present perspective effects of a past light. We men, on the other hand, when in introspecting about

our memories we report visual images, internal speech and the like, are, it would seem, reporting just such present perspectives. That is, we are reporting that in addition to what we take to be the actual independent qualities of the given past object there are also before us, here and now, at the moment of recall, such and such immediate perspective effects. These immediate perspective effects—these visual, auditory, kinesthetic images, or this internal speech—are objectively definable entities which in introspection we intend and sometimes with more and sometimes with less success attain. The degree of our attainment is, however it must be noted, much more difficult to measure than it is in the case of the perceptual intention of perspectives. For images and internal speech, as objective properties, seem to be located inside our nervous systems somewhere. And that makes it difficult for their true values to be independently determined by the experimenter. Hence it is now, given our present techniques, almost impossible for an experimenter to conclude exactly in how far the given introspecting individual is successfully reporting—successfully "attaining"—his actual images and his actual internal speech. But this is not a fundamental difficulty. The account of memorial perspectives, i.e., of visual and other images and of internal speech which now gets into our psychology, while certainly far less reliable and less valid than our accounts of perceptual perspectives, is nonetheless, I would declare, quite as objective.

(2) But let us turn, now, to a brief consideration of the second main subtype of cognitive behavior-readiness, what I called *hypotheses*; or intentions, expectations and attainments as to relations. Suppose a rat be run in a successive discrimination box. Such a box is an apparatus in which the animal has to choose one of two doors at each of four successive choice points. One of the two doors at each such point is lighted and one is dark. The lighted door may be either the one on the left or the one on the right in chance order. Thus at each such point the animal has the possibility of responding either on the basis of light-darkness or on that of right-leftness. Suppose, now further, that it be arranged by the experimenter that the correct choices shall

in a day's series of 10 trials, or 40 choices in all, fall an equal number of times to the left and an equal number of times to the right, and suppose it also be arranged that the correct door be an equal number of times a dark door and an equal number of times a lighted door. Under these conditions it was found by Krechevsky,[21] whose experiments it is I am reporting, that the rat will pick up one systematic way of behaving after another. In the first two or three days he may pick up, say, the propensity of choosing always the right hand doors. But then he will shift sooner or later to some new propensity, to that say, of choosing only the left hand doors; and then still later to that of choosing alternate right and left doors; or he may shift to choosing all the lighted doors, irrespective of side, or all the dark doors, or to choosing alternately light and dark; and so on. Each such systematic propensity will be adopted for a time and then dropped in favor of some other. And, following Krechevsky, we may now define each such intervening condition (or "I") in the organism, behind any one such systematic way of behaving, as an hypothesis.

An hypothesis, behavioristically, in other words, is to be defined as nothing more nor less than a condition in the organism which, while it lasts, produces just such a systematic selectivity in behavior. Further, it appears that such an hypothesis or selectivity is equivalent to an intention or assertion of a specific relation as obtaining in the environment. In the above case these assertions are to the effect that it is such and such types of door which lead on and such and such other types which are closed. The rats assert—hypothesize—that it is the right hand doors, or the left hand doors, or alternate right and left doors or dark doors, or whatever, which, as such, lead on. And when any one such assertion proves incorrect, an animal sooner or later drops it for a new one.

[21] I. Krechevsky, "Hypotheses" versus "Chance" in the pre-solution period in sensory discrimination-learning, *Univ. Calif. Publ. Psychol.*, 1932, *6*, 27–44.

I. Krechevsky, The docile nature of "hypotheses," *J. Comp. Psychol.*, 1933, *15*, 429–433.

I. Krechevsky, The genesis of "hypotheses" in rats, *Univ. Calif. Publ. Psychol.*, 1932, *6*, 45–64.

In the experiment as thus far described, the problem given to the animals was actually insoluble. The correct doors were, that is, determined by chance. And no hypotheses—none of the systematic selectivities in the behaviors of the animals—could prove successful. This meant that during the entire duration of the experiment the rats kept shifting from one hypothesis to another. In other experiments, however, the situation was different. Thus in one case it was arranged that after a rat had once adopted some hypothesis with a given degree of consistency the experimenter then made that hypothesis correct. Under these conditions the animals persisted in their now correct hypothesis throughout the entire remainder of the investigation. Or, again in still another set-up, a certain hypothesis was made correct from the very beginning. In such a case the rats might begin with various wrong hypotheses. But they always ended sooner or later with the single correct one. So much for rats, let us turn now to human beings.

In men, also, an hypothesis is fundamentally, I believe, no more than a similar behavior-readiness. It is a condition or set which produces consistent selectivity of behavior relative to some type of environmental relation. But in men there are, to be sure, complications and refinements. Men can introspect. And when they introspect they tell us that it is not essential to their hypotheses that these issue into actual behavior. Hypotheses, they say, can be purely "mental." Furthermore, as thus mental, they can be "mentally" tested out to see if they are going to work before they go over into actual behavior. And, if they won't seem to work, they can be rejected. Now these introspections concerning hypotheses cannot be completely ruled out of court. They have to be considered further. But, first, it may be well to note that to be thus conscious and introspectable is not always inevitable even for human hypotheses.

Thus, for example, in certain multiple choice experiments with human subjects—a type of experiment very similar to Krechevsky's discrimination box experiment just described with rats—Warner Brown and Miss Whittell[22] found cases in which

[22] W. Brown and F. Whittell, Yerkes' multiple choice method with human adults, *J. Comp. Psychol.*, 1923, *3*, 305–326.

the human subject arrived at the correct solution without, however, himself being able consciously to formulate the nature of his hypothesis. The individual chose the correct door every time (in this case not a door but a counter representing a door) without, however, being able to say what it was that defined his choice. He was not conscious of his hypothesis and yet this hypothesis successfully governed his behavior. We must conclude, therefore, that to be conscious—to be introspectively formulable—is not essential to even human hypotheses. The fundamental essence of any hypothesis is not, it would seem, whether it be conscious or not but the fact that it is a condition in the organism which sets the latter, whether human or subhuman, for a certain systematic selectivity of behavior. In the case of a man this condition may not get over into actual behavior but its significance and meaning is that, if it did get over into behavior, it is such and such behavior which it would determine. An hypothesis is defined by the fact that it would determine the choosing of left-hand doors rather than right-hand doors, or that it would determine the "apple polishing" of some instructors and the avoiding of all others.

Consider now, however, the further problem that an human hypothesis *can* be conscious and also that when conscious it can be held up and tested out before going over into gross behavior. What further points do these facts involve? It seems to me that perhaps there are two such points. The first is similar to that already made for the case of perceptual and memorial differentiations, it is the fact namely that in introspection a man can apparently intend the momentary perspective properties as well as the independent property. And so now in the case of hypotheses, here also a man can apparently in introspection "intend" not the independent relation but rather the momentary perspectives corresponding to that independent. He can intend, that is, the momentary immediate effects in himself—the immediate visual, auditory-kinesthetic, organic images, or whatever they may be which result from the external independent relation. And a large part of what we are wont to demand in the laboratory in the way of introspection is just such a reporting of these immediate perspectives.

But there is a second more important feature involved in hypotheses as conscious,—the fact, already noted, that they can be mentally tested out. We must conceive, that is, that in human beings and perhaps also in some of the lower animals—certainly in the monkeys and the great apes—consciousness allows for a playing off of one intention, one behavior-readiness, against another. It is thus that the possibility of arriving at modified and improved sets of these intentions or behavior-readinesses is achieved. The conscious organism checks his hypothesis against his memories or against his perceptions. And, if necessary, he changes his hypothesis accordingly. He hypothetically expects all dark doors to be open. And then he memorially intends the openness or closedness character of the last dark door. And he remembers, say, that it was not open but closed. The two intentions—the two sets of behavior-readiness—are thus at variance. He therefore corrects the former in the light of the latter. This sort of procedure, which seems to be limited to us men and to perhaps some of the higher animals, is certainly most remarkable. And it needs a lot more in the way of a successful analysis and study than any psychologist has yet succeeded in giving it. I see no reason, however, to suppose that it involves anything of a fundamentally different order from the sort of facts that we have already considered. It is another feature about the nature and functioning of the intervening variables—the intervening behavior-readinesses. But, as such, it can likewise be taken care of, I would declare, in purely constructuralistic logical terms. For the purposes of psychology this capacity of the higher organisms to check up their intentions one against another can not require us to introduce into our account new variables of a non-objective or experiential order. Science is, and must remain, a map and not turn into a re-living even when it has to concern itself with consciousness.

So much for a brief and inadequate consideration of the cognition variables—the differentiations of qualities and the hypotheses as to relations. There is one final set of problems still left over which we cannot attempt to treat now and our answers to which would still leave much to be desired. These are ques-

tions concerning the nature of the functions f_3 which connect the cognition variables to the independent variables and of the function f_2 which connects the cognition variables along with the demand variables to the final behavior B. As regards the functions in the class f_3 we already know some of the answers. They are the laws of sensory capacity, memory, learning and reasoning, which we to some extent now have. But even as regards them there is obviously still much more to be done. And as to the function f_2 we are as yet almost completely in the dark. There is at present one psychologist, Lewin,[23] who, I think, is doing something very important in this direction. But the whole affair is still too complicated for me to attempt to indicate it here.

This completes my argument. It has been long and, I fear, obscure. Therefore in conclusion I would like briefly to enumerate what seem to me to have been the more important points:

(1) Modern psychology and modern physics starting from early noted facts concerning the relativity of sense perception were led to make a distinction between sense data, as immediately given and to be studied by psychology, and real material objects, behind such sense data to be studied by physics.

(2) The more recent work on perception such as that of Brunswik and his students leads us now, however, not to the distinction between sense data and hypothetical reals behind such sense data, but rather to one between the perceptual intention of perspectives and the perceptual intention of independents. And both these types of intention are objectively discoverable and definable logical constructs.

(3) The true dichotomy is not, therefore, between sense data and physical reals but between the logical constructs of both physics and psychology, on the one hand, and the matrix of immediate experience, on the other.

(4) In the case of psychology the logical constructs to be evolved are necessarily those of a behaviorism.

(5) A behaviorism seeks to write the form of the function f_1 which connects the dependent variable—the behavior, B—to

[23] Cf. K. Lewin, "Environmental Forces"—Handbook of Child Psychology, edited by C. Murchison, 2nd rev. edit. Worcester, Clark University Press, 1933.

the independent variables—stimulus, heredity, training, and physiological disequilibriums, S, H, T, and P.

(6) The determination of this final function is, however, at present too difficult and behaviorism breaks its task down into two steps by the insertion of intervening variables $I_a, I_b, I_c, \ldots I_n$.

(7) It appears further, however, that there can be two different types of behaviorism and that the nature of these intervening I's will be conceived differently by each of these types.

(8) The one type, which I have called "molecular behaviorism," and which, as I see it, is more nearly the task of the physiologist than of the psychologist, tries to work out these I's in detailed neurological and glandular terms. The other type, which I have called "molar behaviorism," and which as I see it is the task of the psychologist proper, tries to work out these I's at a more macroscopic level in the guise of "behavior-readinesses." These behavior-readinesses are to be conceived as correlated with the underlying molecular I's but as having their own level of description.

(9) These molar I's or behavior-readinesses would, according to my doctrine, be divided further into "demands" and "cognitions." The demands would then further subdivide into ultimate and subordinate demands and the cognitions into "differentiations" (or intentions as to qualities) and "hypotheses" (or intentions as to relations).

(10) These demands, differentiations and hypotheses are all demonstrated and defined by objective experiments—the sorts of experiments that we can perform with animals as readily as with men. They are, in short, but logical constructs. They are not re-livings of immediate experience. Psychology like physics deserts immediate experience and leaves it for the philosopher, the poet, or the proponent of common sense.

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REVIEWS AND NOTES

Philosophy of Science. By Fulton J. Sheen, Ph.D., D.D., Ll.D., L.H.D. The Bruce Publishing Co., Milwaukee, 1934. Pp. 197 + xxv. $2.75.

This is a volume of many prefaces, a "nihil obstat" and an "imprimatur." Its position (or bias) is, therefore, rendered unusually clear. Leon Noël characterizes the book's aim as "rescuing the reader from the childlike simplicity which takes the results of science for what they assuredly are not, namely, a metaphysical view of the universe." The general editor, J. Husslin, S.J., Ph.D., states that the obvious purpose of the book is "to bring about a more intelligent understanding between modern science and Scholastic philosophy which brings to it the reasoning of Aristotle and St. Thomas, *equally valid for all time.*" Philosophy of science is conceived as the metaphysical interpretation (applied metaphysics) of the empiric facts of science. The author himself tells us what the book is not: "it is not, in any sense, a discussion of scientific methods; the book does not hail Einstein merely because his theory of relativity of space and time smacks of the Scholastic doctrine that both are inseparable from matter."

Dr. Sheen acknowledges two theories of the philosophy of science: the physical theory and the mathematical theory. The former, a remnant of positivism, asserts that no knowledge is possible except the knowledge of phenomena, and is erroneous because it is a denial of metaphysics and of a knowledge of the transcendence of God; the mathematical theory seeks a higher discipline to interpret scientific theory, namely mathematics. Now, a third theory, the metaphysical, called by the Scholastics the philosophy of Nature, goes but a step further than the second in invoking a higher interpretation than the mathematical, namely the first principles of thought which are transcendent to the world and universal in application. Explanation is now at the level of *primary* causes rather than secondary. With Dean Inge he proposes the *philsophia perennis* of Aquinas as the abiding solution.

Dr. Sheen makes a good deal of contrasts like the following;—science seeks *to describe* in terms of the phenomenal, philosophy (of science) to *explain* in terms of the intelligible. In other words, philosophy knows more than facts and declines the perverse utilitarianism of pragmatism. Philosophy, not science, judges facts; there is a higher discipline than experimental verification; philosophy has a higher mission for itself than applying the categories of the material to the spiritual, of the physical to the mental, and the spatio-temporal to the eternal. Philosophy is, therefore, decidedly not the synthesis of the sciences. Yet the author somehow or other makes her the "Queen of the Sciences" after all, in giving a summary of the Scholastic position concerning philosophy and science: "Philosophy and science both deal with being, but from different points of view. Physics, for example, treats of inorganic material being; biology treats of organic being; psychology treats of mental being. But philosophy treats of being as being. There is no particular science which studies universal being or the fullness of all that is, except metaphysics; every other science treats of only a part of that being. The emphasis of physics in the older terminology was on *ens mobile*, but the emphasis of metaphysics is on *ens inquantum est ens*."

The attitude of the author is on the whole conciliatory. He devotes a chapter to an appreciation of the theories he criticizes. And yet he feels that he is refuting the physical theory by the parable that a cat walking through the laboratory sees the facts in the test tubes and probably sees them better in the dark than the scientist himself. Whatever appreciation there is lies in conceding that the two theories are good *methods*, but are not philosophies at all.

The volume sounds most authoritative in its exposition of the scholastic doctrine of science. The presentation, nevertheless, is marred by frequent interruptions to show that the Angelic Doctor had anticipated the philosophic faculty of the College of the City of New York on many precious points, e.g. the contingency of science, and the like. An interesting discussion is given of abstraction as the condition of metaphysics. "Abstraction is that mental process by which the potential intelligibility of the things is rendered actually intelligible by the possessing of the *ratio*, or form, or pattern in the knowing mind. The idea in the mind is the form in the concrete world, except that it is endowed not with concrete, but with mental or *intentional* being as the Scholastics called it. In its content it is quite different from the classified common characteristics of scientific objects, and not to be confused with them." This is

followed by a discourse on Being, its transcendental properties, and the metaphysical judgments concerning it. The jumping electron has nothing to do with the concept of cause, because cause, by its very nature, is metaphysical. Truth does have a capital T. To the question whether the categories of physics, biology and psychology may be applied to philosophy, the answer is a decided "no." "A science with a more restricted object may not apply its principles to a higher science." The dependence is all one-sided. And so by steps to the conclusion that "man's final bliss consists in the supernatural vision of God."

Thus, under the protection of Scholastic philosophy, science can tread the difficult path from the natural to the supernatural. Indeed it is a pleasant rationalization!

W. M. Malisoff.

Science and the Public Mind. By Benjamin C. Gruenberg. McGraw Hill Book Co. Pp. 96 + xi. $2.00.

This volume is an eclectic study of opinion on the vital subject of the "place of science in adult education." "There have been interviews or conferences with some two hundred educators, scientists, publicists," reports the scholarly and energetic author. The diffusion of scientific attitudes is evidently the author's ideal, and bad popularization is his bugaboo. The true scientific spirit will maintain society in health and sanity, if one could only spread it efficiently. One wishes Mr. Gruenberg had more to say as to what the true scientific spirit really is and what it can do out of its immediate province, since so many scientists are obedient to it in the laboratory and are perfect dunces elsewhere. Mr. Gruenberg sees science as the leaven of democracy, but if it is to become "a little learning" spread thin on a colossal scale, may it not become like other democratic afflictions, and perhaps the greatest danger of them all?

W. M. M.

A System of Logistic. By Willard van Orman Quine. Harvard University Press, Cambridge, Mass., 1934. Pp. xii + 204.

The work before us presents the system of *Principia Mathematica* (PM) in a new symbolism. This novelty is partly the cause and partly the effect of a new gain in generality. It enables us to escape one of the limitations of PM and to treat propositional functions without reference to the number of variables they contain. Types of order are discussed in a very general way and the proposition is defined as such a type.

The question then is: Is the new generalization of any further use than to fix the meaning of terms, classes, relation, propositions, etc.? If not, the gain can be only one of symbolic technique, an advantage, however, not to be underestimated.

The theory of types is carried over into the present system without basic modification. The system must therefore share all the weaknesses of that theory. Some day these weaknesses will be met by the construction of a logic that is perfectly general, a logic that will render systematically the properties of those modal distinctions which PM wipes out.

Dr. Quine's book corresponds to an intellectual effort of the highest order. If its general notions can be applied to a logic that sets no limitations on the meaning of its variables, then its effort will not have been wasted.

HENRY BRADFORD SMITH.

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BOOKS RECEIVED

Materialism the Scientific Bias. By Henry Felix Mins, Jr. For sale at The Journal of Philosophy. Pp. 120. $1.00.

Passivity and Rationalization. By Nathan Fialko. John Lowell Pratt. 1935. Pp. 214. $2.50.

The Dilemma of Modern Physics: Waves or Particles? By Donald Everett Richmond. G. P. Putnam's Sons, 1935. 120 + xiii pp. $2.00.

Pareto's General Sociology. A Physiologist's Interpretation. By Lawrence J. Henderson. Harvard University Press, 1935. Pp. 119. $1.25.

ANNOUNCEMENTS

INTERNATIONAL CONGRESS FOR UNITY OF SCIENCE

The first of the international congresses for the unity of science will take place in Paris Sept. 15–23, and will be devoted to the topic of the philosophy of science.

The aim of these congresses is to make known and to further the logistic investigations of Russell, the studies represented by the contributions to such journals as Erkenntnis and the Philosophy of Science, the writings of the Wiener Kreis, the metamathematical approach of David Hilbert, the metalogic of the Polish school, the scientific empiricism of

the American thinkers, and all movements of similar temper. Since a main task is to present the essential nature of the scientific enterprise and habit of mind ("the logic of science," "the logical syntax of the language of science," "scientific empiricism" and the like) the first congress is to be devoted to the philosophy of science.

Among the members of the organizing committee are Bridgman, Carnap, Enriquez, Frank, Frechet, Gaultier, Jorgensen, Hadamard, Janet, Kotarbinski, Lukasiewicz, C. I. Lewis, Morris, Neurath, Nicolle, Reichenbach, Rougier, Schlick.

Communications may be addressed to the secretary, Dr. Otto Neurath, Mundaneum Institute, 267 Obrechtstraat, The Hague, Holland.

SEMINARS IN THE PHILOSOPHY OF SCIENCE

The Editor invites correspondence concerning the organization of seminars in the philosophy of science. There is available for distribution a résumé of a seminar given by him at the University of Pennsylvania on this subject. The information given by correspondents will be summarized and reported in this JOURNAL from time to time.

A NEW BUDGET OF PARADOXES

(6) NOTES FROM A CASE-BOOK

Case 8. Emulsion-maker, specialist in non-sequiturs. Title of work: Behold! The Grand Problem No Longer Unsolved (The Circle Squared Beyond Refutation) 1931—mathematical and geometrical demonstrations disproving numerous theorems, problems, postulates, corolleries (sic), . . . and rules hitherto unknown in mathematical and geometrical science, naturally growing out of the extraordinary and significant discoveries of a LACKING LINK by C. T. F. who was a Citizen of Brooklyn, N. Y., U. S. A., in the demonstration of the world renowned PYTHAGOREAN PROBLEM utterly disproving its absolute truth, although demonstrated as such for twenty-four centuries . . . by C. T. H., 33° Mason, a citizen of Cleveland, Ohio, U. S. A. Burkeley (sic) and Piscal (sic) are the renegade philosophers who had supported our author, who finds that "mathematicians may be divided into two classes . . . spiritual and material. The first have a knowledge or some conception of mathematical principles, but the second have none whatever."

Case 9. Magician, illusionist. Title: The Trisection of the Angle by Plane Geometry Verified by Trigonometry by J. W., B. A., M. D., Edin. "It will be noticed that occasionally the last figure (the seventh decimal) differs by 1 from the exact result given in the Tables: sometimes the sine differs to this extent, while the cosine is exactly correct," admits J. W., but is not the least bothered by it, since he claims to have carried out the conditions set by no other than Augustus de Morgan himself. The author does not employ the number of the beast, but does express a nostalgia for bigger and better logarithms!

W. M. M.